DOCUMENT RESUME

ED 377 335 CE 067 761

AUTHOR	Quint, Janet C.; And Others
TITLE	New Chance: Interim Findings on a Comprehensive Program for Disadvantaged Young Mothers and Their Children.
INSTITUTION	Manpower Demonstration Research Corp., New York, N.Y.
PUB DATE	Sep 94
NOTE	412p.; For related reports, see ED 341 798 and ED 367 929.
PUB TYPE	Reports - Research/Technical (143)
EDRS PRICE	MF01/PC17 Plus Postage.
DESCRIPTORS	Adult Basic Education; Comparative Analysis; Day Care; *Demonstration Programs; *Disadvantaged Youth; *Dropout Programs; Dropouts; *Early Parenthood; Job Training; *Mothers; National Surveys; Outcomes of Education; Participant Characteristics; Program Development; *Program Effectiveness; Services; Tables (Data); Welfare Recipients; Well Being; Youth Programs
IDENTIFIERS	*New Chance; Welfare to Work Programs

ABSTRACT

The New Chance Demonstration, which operated at 16 locations in 10 states in 1989-1992, offered comprehensive education, training, and assistance to teenage mothers who had dropped out of high school. The demonstration, which was designed to increase the young mothers' well-being and long-term self-sufficiency, was subjected to an interim evaluation during which 2,322 women were randomly assigned to either an experimental group (those women allowed to enroll in New Chance) or a control group (those without access to New Chance but with access to alternative services in their communities). Structured interviews were administered to each woman 18 months and again at 42 months after she entered the research sample. The evaluation results were mixed. Many enrollees managed to attain a General Educational Development certificate, used better-quality child care, and demonstrated modestly improved parenting skills; however, New Chance participants also had high rates of repeat pregnancy, their program attendance was sporadic, and more than 80% remained on welfare 18 months after enrolling in New Chance. (Appended are a research group comparison, analysis of survey nonresponse, New Chance site profiles, and characteristics of New Chance enrollees by site. Contains 153 references, a list of 84 selected Manpower Demonstration Research Corporation publications, and 87 tables/figures/boxes.) (MN)

****
* Reproductions supplied by EDRS are the best that can be made *
* from the original document. *
****

CE

ED 377 335

New Chance

Interim Findings on a Comprehensive Program for Disadvantaged Young Mothers and Their Children

U.S. DEPARTMENT OF EDUCATION
Office of Educational Research and Improvement
EDUCATIONAL RESOURCES INFORMATION CENTER (ERIC)
☑ This document has been reproduced as received from the person or organization originating it.
☐ Minor changes have been made to improve reproduction quality.
- Points of view or opinions stated in this document do not necessarily represent official OERI position or policy.

Janet C. Quint
Denise F. Polit
Hans Bos
George Cave

"PERMISSION TO REPRODUCE THIS MATERIAL HAS BEEN GRANTED BY
J S Greissman
TO THE EDUCATIONAL RESOURCES INFORMATION CENTER (ERIC)."

September 1994

Manpower
Demonstration
Research
Corporation

MDRC

CE067761

BEST COPY AVAILABLE

BOARD OF DIRECTORS

RICHARD P. NATHAN, *Chairman*
Provost, Rockefeller College
State University of New York
Director, Rockefeller Institute
of Government

PAUL H. O'NEILL, *Treasurer*
Chairman and CEO
Alcoa

ELI GINZBERG, *Chairman Emeritus*
Director
The Eisenhower Center for the
Conservation of Human Resources
Columbia University

REBECCA M. BLANK
Associate Professor of Economics
Northwestern University

ANTONIA HERNANDEZ
President and General Counsel
Mexican American Legal Defense and
Educational Fund

ALAN KISTLER
President
Human Resources Development Institute
AFL-CIO

RICHARD J. MURNANE
Professor of Education
Graduate School of Education
Harvard University

RUDOLPH G. PENNER
Director of Economic Studies
Policy Economics Group
KPMG Peat Marwick

FRANKLIN D. RAINES
Vice Chairman
Fannie Mae

ROBERT SOLOW
Institute Professor
Massachusetts Institute of Technology

GILBERT STEINER
Senior Fellow
Brookings Institution

MITCHELL SVIRIDOFF
Professor Emeritus and Senior Fellow
Community Development Research Center
New School for Social Research

WILLIAM JULIUS WILSON
Lucy Flower University Professor
of Sociology and Public Policy
The University of Chicago

WILLIAM S. WOODSIDE
Chairman, Sky Chefs, Inc.
Former Chairman and CEO,
Primerica Corporation

JUDITH M. GUERON
President
Manpower Demonstration Research Corporation

MDRC

New Chance

Interim Findings on a Comprehensive Program for Disadvantaged Young Mothers and Their Children

Janet C. Quint
Denise F. Polit
Hans Bos
George Cave

September 1994

Manpower
Demonstration
Research
Corporation

MDRC

Funders of the New Chance Demonstration

U.S. Department of Labor
Ford Foundation
W. K. Kellogg Foundation
DeWitt Wallace–Reader's Digest Fund
Meyer Memorial Trust
The UPS Foundation
Charles Stewart Mott Foundation
The Pew Charitable Trusts
Stuart Foundations
William T. Grant Foundation
The Skillman Foundation
The David and Lucile Packard Foundation
AT&T Foundation
The Bush Foundation
Foundation for Child Development
Exxon Corporation
The Chase Manhattan Bank, N.A.
Koret Foundation
ARCO Foundation
GE Foundation
National Commission for Employment Policy
Mary Reynolds Babcock Foundation
The Allstate Foundation
Neighborhood Reinvestment Corporation
Honeywell Foundation
Grand Metropolitan Foundation
Kaiser Permanente
Anonymous Funder

Dissemination of MDRC publications is also supported by MDRC's Public Policy Outreach funders: the Ford Foundation, the Ambrose Monell Foundation, and the Alcoa Foundation.

The findings and conclusions presented in this report do not necessarily represent the official positions or policies of the funders.

To preserve confidentiality, names and certain minor identifying details have been changed when describing specific members of the research sample.

Library of Congress Cataloging-in-Publication Data

New Chance: Interim findings on a comprehensive program for disadvantaged young mothers and their children / Janet C. Quint ... [et al.].
p. cm.
Includes bibliographical references.
1. Teenage mothers–Services for–United States–Evaluation. 2. Unmarried mothers–Services for–United States–Evaluation. 3. Welfare recipients–Services for–United States–Evaluation. I. Quint, Janet. II. Manpower Demonstration Research Corporation.
HV700.5.N487 1994
362.82–dc20 94-21378
CIP

Copyright © 1994 by the Manpower Demonstration Research Corporation

ACKNOWLEDGMENTS

This report would not have been possible without the cooperation of the young women in the research sample, who shared information about their experiences and beliefs. Equally indispensable were the commitment and cooperation of the staff at the 16 New Chance sites. They regularly provided both quantitative and qualitative information on program operations, and their thoughtful comments on an earlier draft of the report helped us to integrate the perspectives of front-line staff.

We have also benefited from the guidance of MDRC's Board of Directors Committee on Adolescent Parent Studies, chaired by Gilbert Steiner and composed of several Board members and other experts in the field. In particular, Lorraine Klerman, Joyce Ladner, Kristin Moore, Richard Murnane, Judith Musick, Ross Parke, and William Julius Wilson offered useful insights on both the substance and tone of the report.

The report owes much to the efforts of Carolyn Eldred and other staff at Temple University's Institute for Social Research, who conducted, cleaned, and compiled the 18-month interviews with sample members on which the report is based.

Numerous MDRC staff were important to our work on this report. As New Chance project director, Robert C. Granger has provided wise guidance, review, and ongoing support regarding all aspects of the research. Judith M. Gueron, Robert J. Ivry, and Fred Doolittle also provided helpful critiques of the drafts.

With input from David Long, Barbara Fink and Mary Farrell accomplished the massive task of collecting and analyzing detailed cost data from the 16 program sites. In addition, along with Marilyn Price, Ms. Fink conducted a study of the quality of child care delivered by the sites. Marilyn Price, Sharon Rowser, and James Healy provided ongoing information about site activities and drafted the site profiles that appear in Appendix D.

Vicki Hong processed the data, and Karen Trister attended to the many details of report and table production. Juanita Vega-Chetcuti produced the data files on program participation.

Judith Greissman edited the report, assisted by Michael Wilde. Word processing was done by Patt Pontevolpe and Stephanie Cowell.

The Authors

PREFACE

The New Chance Demonstration, and this report, focuses on the population of greatest concern in the current welfare reform debate: young women who have children as teenagers and are high school dropouts. New Chance is a voluntary demonstration project that provides comprehensive education, training, and other services intended to increase the long-term self-sufficiency and well-being of these mothers and their children. While this approach is very different from that expressed in various large-scale welfare reform proposals, its lessons on the complexity of young lives lived in poverty challenge all who are interested in change.

The demonstration was developed in the mid-1980s, when the problem of unwed teenage childbearing was growing but solutions were lacking. Research showed that young mothers who were high school dropouts constituted the group at highest risk of long-term welfare receipt, and that they were unlikely to be able to earn more than they received on welfare unless they acquired more skills. A number of funders and program operators embraced the New Chance approach as one that promised to address both the needs of young families and society's concern with the increasing rates and costs of out-of-wedlock births. The findings presented in this report speak to that concern and also to the related issues of targeting scarce welfare reform dollars, preparing young school dropouts for work, assisting highly disadvantaged children of teen mothers, and improving the way schools serve this population.

The study of New Chance is one of the few large-scale, rigorous evaluations of programs designed to change the outcomes for this population and, while the results reported here are very much a story in progress, the findings in terms of the program's short-term objectives are mixed: a substantial increase in educational attainment (acquisition of a GED, which is frequently a prerequisite for occupational training programs), increased use of good-quality child care, and a modest improvement in participants' parenting skills, balanced against high rates of repeat pregnancy, sporadic program attendance, and the finding that over 80 percent of the young mothers were on welfare 18 months after they enrolled in New Chance.

A companion report published earlier in 1994, and based on in-depth interviews with 50 former New Chance enrollees, pointed to some of the circumstances behind this behavior: jobs found and lost, unplanned pregnancies, ambivalence about the balance between work and parenting responsibilities, and the important role played by family members, partners, and peers in supporting or undermining the young women's efforts to move forward. It offered moving testimony that behind the statistics and public policies are a group of young women determined to build a better life for their children but who, with few resources and little support, are frequently stymied in their progress.

The demonstration's final report, based on 42 months of follow-up, will analyze whether the increased GEDs and use of New Chance services have translated into gains in employment, reductions in time on welfare, and improved outcomes for the children of program participants. But these sobering interim results already speak to the importance of developing earlier interventions that succeed in preventing teen pregnancies, as well as the likelihood that it is going to take time for disadvantaged young women to become self-sufficient once they are mothers. While New Chance is only one possible approach, these findings caution against expecting any easy solution to the consequences of unwed teenage childbearing and school dropout.

The New Chance Demonstration is a remarkable partnership of many funders, states, and local programs. This report is greatly indebted for their support and the cooperation of the young women in the New Chance study.

Judith M. Gueron
President

CONTENTS

PAGE

ACKNOWLEDGMENTS iii
PREFACE v
TABLES, FIGURES, AND BOXES x
ABBREVIATIONS xvi
EXECUTIVE SUMMARY xvii

CHAPTER

1 INTRODUCTION 1

- I. The New Chance Demonstration: An Overview 1
- II. Adolescent Childbearing: The Issues 6
- III. Young Mothers and Welfare: The Problem and the Policy Response 8
- IV. The New Chance Program Model 11
- V. The Research Agenda and Associated Issues 14
- VI. Lessons from Other Programs for Young Mothers 16
- VII. The Contents and Organization of This Report 19

2 RESEARCH DESIGN, SAMPLES, DATA SOURCES, AND ANALYSIS PLAN . 20

- I. Introduction 20
- II. New Chance Participation and Impacts: A Conceptual Model 20
- III. The Study Design 23
- IV. The Research Sample 25
- V. Data Sources for This Report 34
- VI. The Structure of the Analysis 36

3 IMPLEMENTING NEW CHANCE 40

- I. Introduction 40
- II. The New Chance Structure and Components 42
- III. Participation in New Chance Activities 58
- IV. The Quality of Program-Related Child Care 73
- V. Participants' Assessments of the Program 76
- VI. The Cost of New Chance 76

CHAPTER PAGE

4 SERVICE RECEIPT BY EXPERIMENTAL AND CONTROL GROUP MEMBERS 85

I. Introduction 85
II. Impacts on Participation in Education Activities 87
III. Impacts on Participation in Employment-Related Activities 96
IV. Impacts on Participation in Parenting and Personal Development Activities 104
V. Sample Members' Ratings of the Services They Received 104
VI. New Chance and Other Programs Compared 109

5 IMPACTS ON EDUCATIONAL ATTAINMENT AND ACHIEVEMENT 113

I. Introduction 113
II. Impacts on the Attainment of Education Credentials 115
III. Impacts on Literacy 125
IV. New Chance and Other Programs Compared 128

6 IMPACTS ON LIVING ARRANGEMENTS, FERTILITY, AND HEALTH 131

I. Introduction 131
II. Impacts on Living Arrangements and Marriage 132
III. Impacts on Fertility and Contraception 136
IV. Impacts on Health-Related Outcomes 155
V. Impacts on Emotional Well-Being 158

7 IMPACTS ON PARENTING, CHILD CARE, AND CHILD HEALTH 163

I. Introduction 163
II. Impacts on Parenting and the Home Environment 165
III. Impacts on Child Care 178
IV. Impacts on Child Health 187

8 IMPACTS ON EMPLOYMENT, EARNINGS, WELFARE RECEIPT, AND FAMILY INCOME 192

I. Introduction 192
II. Impacts on Employment Rates 193
III. Impacts on Hours and Weeks Worked 195
IV. Impacts on Earnings 197
V. Job Characteristics, Job Tenure and Job Loss, and Efforts to Find Work 202
VI. Impacts on Welfare Receipt and Family Income 208
VII. Impacts on Skill-Building Activity 212
VIII. New Chance and Other Programs Compared 215

CHAPTER PAGE

9 SHORT-TERM ANSWERS, LONG-TERM QUESTIONS 217

I. Introduction 217
II. The Findings in Summary 217
III. Changing Lives, Uncertain Directions 222
IV. New Chance and Public Policy 229
V. Implications for Program Design 235

APPENDICES 241

Appendix A A Comparison of Research Groups 243
Appendix B Analysis of Survey Nonresponse 253
Appendix C The Environment of New Chance Sites 265
Appendix D New Chance Site Profiles 269
Appendix E Characteristics of New Chance Enrollees, by Site 291

REFERENCES 299

SELECTED MDRC PUBLICATIONS 307

TABLES, FIGURES, AND BOXES

TABLE PAGE

1 The New Chance Program Operators xviii

2 Selected Characteristics of the New Chance Sample at Random Assignment xxvi

3 Impacts of New Chance on Services Received Within 18 Months After Random Assignment xxxiv

4 Impacts of New Chance on Educational Attainment and Achievement at 18 Months After Random Assignment xxxv

5 Impacts of New Chance on Fertility-Related Outcomes Through 18 Months After Random Assignment xxxvii

6 Impacts of New Chance on Employment and Welfare Receipt Through 18 Months After Random Assignment xlii

1.1 The New Chance Program Operators 2

1.2 Funders of the New Chance Demonstration 5

1.3 The New Chance Model 12

2.1 Selected Characteristics of the New Chance Sample at Random Assignment 27

2.2 New Chance 18-Month Follow-Up Survey Modules 35

3.1 Schedule of Phase I New Chance Components, by Site 46

3.2 Participation Rates, Hours of Participation, and Months of Activity for New Chance Experimentals Within 18 Months After Random Assignment 59

3.3 Average Hours of Participation of New Chance Experimentals Within 18 Months After Random Assignment, by Characteristics at Random Assignment 62

3.4 Participation in New Chance Within 18 Months After Random Assignment, by Site 65

3.5 Percentage and Average Hours of Participation of New Chance Experimentals in Skills Training or a Work Internship Within 18 Months After Random Assignment, by Site 67

TABLE **PAGE**

3.6 Percentage and Total Average Months of Participation of New Chance Experimentals in Phase II Components Within 18 Months After Random Assignment, by Site and GED Receipt at 18 Months 69

3.7 Average Child-to-Staff Ratios and Average Group Size for New Chance Child Care Centers, Accreditation Standards, and Two Comparison Child Care Samples 75

3.8 Experimentals' Ratings of New Chance Program Features and Sites 77

3.9 Total Cost of New Chance per Experimental, by Site, Agency, and Program Phase 79

3.10 Cost of New Chance Components per Experimental, by Site 81

3.11 Summary of New Chance Implementation, by Site 83

4.1 Impacts of New Chance on Participation in Education Programs Within 18 Months After Random Assignment 88

4.2 Impacts of New Chance on Number of Weeks Participated in Any Education Program Within 18 Months After Random Assignment, by Subgroup 93

4.3 Impacts of New Chance on Number of Weeks Participated in Any Education Program Within 18 Months After Random Assignment, by Site 97

4.4 Impacts of New Chance on Participation in Employment-Related Activities Within 18 Months After Random Assignment 98

4.5 Impacts of New Chance on Number of Weeks Participated in Skills Training Within 18 Months After Random Assignment, by Subgroup 101

4.6 Impacts of New Chance on Number of Weeks Participated in Skills Training Within 18 Months After Random Assignment, by Site 105

4.7 Impacts of New Chance on Participation in Classes on Parenting, Family Planning, Health, and Life Skills Within 18 Months After Random Assignment 106

4.8 How Experimentals and Controls Rated Various Activities in Which They Participated 108

TABLE **PAGE**

4.9 A Comparison of Program Impacts on Service Receipt in New Chance and Other Selected Programs for Young Mothers 110

5.1 Impacts of New Chance on Educational Attainment at or Within 12 and 18 Months After Random Assignment 116

5.2 Impacts of New Chance on Receipt of a GED or High School Diploma Within 18 Months After Random Assignment, by Subgroup 119

5.3 Impacts of New Chance on Receipt of a GED or High School Diploma Within 18 Months After Random Assignment, by Site 122

5.4 Impacts of New Chance on Literacy at 18 Months After Random Assignment 127

6.1 Impacts of New Chance on Household Living Arrangements at 18 Months After Random Assignment 134

6.2 Net Impacts of New Chance on Pregnancy and Childbearing at or Within 18 Months After Random Assignment 138

6.3 Impacts of New Chance on Birth Control and Fertility Expectations at 18 Months After Random Assignment 140

6.4 Impacts of New Chance on Pregnancy Within 18 Months After Random Assignment, by Subgroup 144

6.5 Impacts of New Chance on Pregnancy Within 18 Months After Random Assignment, by Site 148

6.6 Impacts of New Chance on Pregnancy Combined with Other Outcomes at 18 Months After Random Assignment 151

6.7 A Comparison of Fertility-Related Impacts in New Chance and Selected Other Programs 153

6.8 Impacts of New Chance on Health-Related Outcomes at or Within 18 Months After Random Assignment 157

6.9 Impacts of New Chance on Indicators of Emotional Well-Being at 18 Months After Random Assignment 160

7.1 Impacts of New Chance on Home Environment and Parenting Scales at 18 Months After Random Assignment 169

7.2 Impacts of New Chance on the Emotional Support Subscale of the HOME Scale at 18 Months After Random Assignment, by Subgroup 173

TABLE **PAGE**

7.3 Impacts of New Chance on Emotional Support Subscale Scores of the HOME Scale at 18 Months After Random Assignment, by Site 176

7.4 Impacts of New Chance on Child Care Use at or Within 18 Months After Random Assignment, for the Focal Child 179

7.5 Impacts of New Chance on Use of a Day Care Center or Preschool Within 18 Months After Random Assignment, by Subgroup 184

7.6 Impacts of New Chance on Use of a Day Care Center or Preschool Within 18 Months After Random Assignment, by Site 188

7.7 Impacts of New Chance on Children's Health and Health Care at or Within 18 Months After Random Assignment 190

8.1 Impacts of New Chance on Employment Rates Within 18 Months After Random Assignment 194

8.2 Impacts of New Chance on Hours and Weeks Worked Within 18 Months After Random Assignment 196

8.3 Impacts of New Chance on Earnings Within 18 Months After Random Assignment 198

8.4 Impacts of New Chance on Average Earnings Within 18 Months After Random Assignment, by Subgroup 199

8.5 Impacts of New Chance on Average Earnings Within 18 Months After Random Assignment, by Site 203

8.6 Impacts of New Chance on AFDC Receipt Within 18 Months After Random Assignment 209

8.7 Selected Impacts of New Chance on Income at 18 Months After Random Assignment 211

8.8 Impacts of New Chance on Skill-Building Activity Within 18 Months After Random Assignment 213

8.9 A Comparison of Impacts on Employment, Earnings, and Welfare Receipt in New Chance and Selected Other Demonstrations for Young Mothers 216

9.1 Selected Impacts of New Chance at or Within 18 Months After Random Assignment 219

TABLE PAGE

9.2 Outcomes for New Chance Experimentals, by Living Arrangement at 18 Months 225

9.3 Impacts of New Chance at or Within 18 Months After Random Assignment for Sample Members Living and Not Living with Their Mothers or Other Adults at Random Assignment 234

A.1 Selected Characteristics of the New Chance Sample at Random Assignment, by Research Group 245

A.2 Estimated Regression Coefficients for the Probability of Assignment to the Experimental Group 251

B.1 Survey Response Rates, by Research Group and Site 255

B.2 Selected Characteristics of the New Chance Sample at Random Assignment, by Survey Respondent Subsample 256

B.3 Estimated Regression Coefficients for the Probability of Being a Responder to the 18-Month Survey 262

C.1 The Environment of New Chance Sites 266

E.1 Characteristics of New Chance Enrollees, by Site 292

FIGURE

2.1 A Simplified Conceptual Model of the Effects of New Chance 21

2.2 Steps in the Intake and Random Assignment of the New Chance Research Sample 24

3.1 Percentage Distribution of the Cost of New Chance Components 82

4.1 Percentage of Experimentals and Controls Attending Any Education Program, by Month After Random Assignment 90

7.1 Use of "Market" Child Care (a Day Care Center, Preschool, or Paid Babysitter/Family Day Care Home), by Month After Random Assignment 182

7.2 Use of Child Care Provided by a Grandparent, Another Relative, or the Mother's Husband or Partner, by Month After Random Assignment 182

8.1 Job Tenure Among Experimentals and Controls, by Month After the Start of a Job 205

8.2 Percentage of Experimentals and Controls Employed or in Education, Training, or a Work Internship, by Month After Random Assignment 214

BOX **PAGE**

Lives in Flux xxvii

Factors Influencing Contraceptive Behavior:
One Young Woman's Story xxxviii

Experiences Working and Looking for Work xliv

Portraits in Diversity 33

How One Site Put the Program Model in Place 43

How One Program Structured Employment-Related Services 70

Why One New Chance Participant Did Not Use Birth Control 142

How One Program Delivered the Message on Repeat Pregnancies 149

Problems at Work (and at Home) 207

ABBREVIATIONS

AFDC	Aid to Families with Dependent Children
CES-D	Center for Epidemiological Studies Depression Scale
DLC	Difficult Life Circumstances Scale
ECERS	Early Childhood Environment Rating Scale
GAIN	California's Greater Avenues for Independence Program
GED	General Educational Development certification
HOME	Home Observation for Measurement of the Environment Scale
HOME-SF	Home Observation for Measurement of the Environment Scale – Short Form
ITERS	Infant and Toddler Environment Rating Scale
JOBS	Job Opportunities and Basic Skills Training Program
JTPA	Job Training Partnership Act (1982)
LEAP	Ohio's Learning, Earning, and Parenting Program
LSO	Life Skills and Opportunities curriculum
MDRC	Manpower Demonstration Research Corporation
MIS	Management Information System
NAEYC	National Association for the Education of Young Children
NCCSS	National Child Care Staffing Study
NLSY	National Longitudinal Study of Youth
PCS	Profile of Child Care Settings
PIC	Private Industry Council
PSID	Panel Study of Income Dynamics
SSI	Supplemental Security Income
STD	sexually transmitted disease
TABE	Tests of Adult Basic Education
TALS	Test of Applied Literacy Skills

EXECUTIVE SUMMARY

New Chance was a national demonstration program operated between 1989 and 1992 at 16 locations in 10 states. The demonstration, supported by a broad consortium of public and private funders, tested a program model intended to improve the economic prospects and overall well-being of low-income young mothers and their children through a comprehensive and intensive set of services. New Chance targeted a group important to much of the current debate on welfare reform – families headed by young mothers (aged 16 to 22) who gave birth during their teenage years and were receiving Aid to Families with Dependent Children (AFDC, the main cash welfare program) – and, within this group, focused on those who were especially disadvantaged by virtue of being high school dropouts. The unmarried status of these young mothers and the educational barriers they confronted placed them at high risk of long-term welfare receipt and economic hardship.

The 16 New Chance programs (also referred to in this report as the research "sites") primarily served young women who volunteered for the program.[1] As shown in Table 1, they were operated by community service organizations, schools and school districts, a community college, a Private Industry Council (the nonprofit entity that administers funding under the federal Job Training Partnership Act, JTPA), and an agency overseen by the county government; one program represented a unique collaboration between a school district and the Job Corps. As also shown in the table, the operators of the New Chance programs had diverse service traditions. The program model, demonstration, and evaluation plan were developed by the Manpower Demonstration Research Corporation (MDRC), a private nonprofit organization that tests initiatives to improve the well-being and self-sufficiency of poor people.

This report, the third of the demonstration, summarizes the research findings on the early (18-month) effects of New Chance.[2] The evaluation includes a comparison of 2,322 women who were randomly assigned to either an *experimental* group (who were allowed to enroll in New Chance) or a *control* group (who did not have access to New Chance services, but many of whom were able to find alternative services in their communities). To determine the effectiveness of New Chance, the *differences* between the two groups of women (often referred to in this kind of research as the *impacts* of the program) are being examined through a structured survey interview at 18 months and 42 months after each young woman entered the research sample. This report also updates the program implementation and cost findings described in the first (1991) report. The evaluation's final report,

---

[1] Currently, all but three of the 16 programs are still in operation. The Chula Vista (California), Bronx (New York), and Inglewood (California) New Chance programs closed because of funding difficulties. These closings did not materially affect the program experiences of the young women at these sites for whom data are presented in this report. This report has been written mostly in the past tense because it describes structures and activities as they existed or occurred during the time period covered by this study.

[2] The two previous reports were: Janet Quint, Barbara Fink, and Sharon Rowser, *New Chance: Implementing a Comprehensive Program for Disadvantaged Young Mothers and Their Children* (New York: MDRC, 1991), and Janet Quint and Judith Musick, *Lives of Promise, Lives of Pain: Young Mothers After New Chance* (New York: MDRC, 1994).

TABLE 1

THE NEW CHANCE PROGRAM OPERATORS

Location	Program Operator	Type of Organization	Prior Emphasis
California (Chula Vista)	Del Rey Center, Sweetwater Union High School District (a)	Adult school	Adult education
California (Inglewood)	Youth and Family Center (a)	Community service organization	Counseling, health services
California (San Jose)	Independence Adult Center, East Side Union High School District	Adult school	Education
Colorado (Denver)	Technical Education Center-North Campus, Community College of Denver	Community college	Adult education, occupational skills training
Florida (Jacksonville)	The Bridge Family Health Services, Inc.	Community service organization	Family planning, health services, tutoring
Illinois (Chicago Heights)	Aunt Martha's Youth Service Center, Inc. (b)	Community service organization	Counseling, education, employment preparation, health services
Kentucky (Lexington)	The Family Care Center (c)	Agency overseen by county government	Prevention and treatment of child abuse and neglect
Michigan (Detroit)	Development Centers, Inc., Community Mental Health Center	Community service organization	Mental health services
Minnesota (Minneapolis)	RESOURCE, Inc. (d)	Community service organization	Occupational skills training, employment preparation
New York (Bronx)	National Puerto Rican Forum, Inc. (a)	Community service organization	Adult education, English as a Second Language, occupational skills training
New York (Harlem)	Mid-Manhattan Adult Learning Center Office of Adult and Continuing Education New York City Board of Education	Adult school	Adult basic education, GED preparation, occupational skills training
Oregon (Portland)	PIVOT-New Chance Program Portland Public Schools	School-Job Corps collaboration	Education
Oregon (Salem)	Teen Parent Program The YWCA of Salem	Community service organization	Recreation, education, child care, counseling, health services
Pennsylvania (Allentown)	Expectant and Parenting Youth Program Private Industry Council of Lehigh Valley	Private Industry Council	Education, life skills, personal development
Pennsylvania (Philadelphia)	Lutheran Settlement House Women's Program	Community service organization	Adult education, life skills
Pennsylvania (Pittsburgh)	Pittsburgh in Partnership with Parents, Hill House Association	Community service organization	Comprehensive services for teenage parents

NOTES: (a) A New Chance program is no longer in operation at this site.

(b) This agency is now located in Park Forest, Illinois.

(c) The Family Care Center is a semiautonomous agency under the oversight of the Lexington-Fayette Urban County Government's Department of Social Services' Division of Family Services.

(d) This agency was formerly named Multi Resource Centers, Inc.

which is scheduled for completion in 1996, will address the longer-term impacts of New Chance and its cost-effectiveness; at that point, the program's effectiveness in increasing employment and reducing welfare receipt, and in improving outcomes for enrollees' children, can be better assessed.

Until relatively recently, teenage mothers with young children were largely ignored by welfare employment programs, which have traditionally served mothers of children who are at least of school age. Teenage mothers have also been underrepresented in programs funded under the Job Training Partnership Act (JTPA), the federal legislation authorizing employment training for youths and adults. As a result, at the time that New Chance was developed, there was little solid information about the kinds of programs and services that are effective in promoting self-sufficiency in this population.

The findings of this study and of other recent evaluations contribute to a growing but still small body of reliable evidence concerning the effects of interventions intended to promote self-sufficiency among young welfare mothers.[3] Although these interventions have targeted different groups within this population and have adopted different approaches, their experiences indicate a number of common themes and lessons. First, young mothers participating in these programs have confronted many serious obstacles to advancement, some psychological in nature, others related to dysfunctional families, dangerous schools and neighborhoods, and other factors in their social, physical, and economic environments. Next, securing high levels of attendance has been an issue in nearly all the programs; furthermore, none of the programs has been successful in delaying repeat childbearing. Moreover, while programs have had varying records in helping young mothers to secure education credentials and employment, in all cases the majority of enrollees remained on welfare at follow-up, and no program has enabled them to escape poverty to any meaningful extent. In addition, programs have generally been less successful with those young mothers who dropped out of school before program enrollment. Finally, the studies suggest that long-term follow-up is critical to determining the effectiveness of programs designed for young people, who may be better able to capitalize on what they have learned in these programs after they have gained greater maturity and their lives have become more stable.

An Overview of the Findings

This report contains findings on the effectiveness of New Chance at a relatively early point – 18 months – after the young mothers entered the research sample. Since only 12 percent of the experimentals were still active in the program at that point, the report presents reasonably complete information on such in-program outcomes as GED attainment and child care use. It also contains data about outcomes on which New Chance sought to have an immediate effect, such as parenting and contraceptive behavior, although the program's long-term effects in these areas may differ from the short-term findings. Data on employment and welfare receipt are included in the interest of completeness, but because in the main New Chance sought to effect changes in these areas over time, conclusions about the program's effectiveness in increasing employment and reducing reliance on AFDC await analysis of the 42-month data.

---

[3] See Dan Bloom et al., *LEAP: Interim Findings on a Welfare Initiative to Improve School Attendance Among Teenage Parents* (New York: Manpower Demonstration Research Corporation, 1993); Rebecca Maynard, Walter Nicholson, and Anu Rangarajan, *Breaking the Cycle of Poverty: The Effectiveness of Mandatory Services for Welfare-Dependent Teenage Parents* (Princeton, N.J.: Mathematica Policy Research, 1993); George Cave et al., *JOBSTART: Final Report on a Program for School Dropouts* (New York: Manpower Demonstration Research Corporation, 1993); and Denise Polit, Janet Quint, and James Riccio, *The Challenge of Serving Teenage Mothers: Lessons from Project Redirection* (New York: Manpower Demonstration Research Corporation, 1988).

With regard to implementation, the 16 New Chance sites were successful in implementing the intended New Chance model, particularly with respect to early program components such as education, parenting, personal development services, and child care. The sites were able to recruit the especially disadvantaged segment of the young mother population the program aimed to serve, and were generally successful in meeting recruitment goals. However, while most women in the experimental group participated in many New Chance services and gave the program high ratings, absenteeism and early terminations resulted in a program experience that, for many, proved to be briefer, less intensive, and less employment-focused than had been planned. Moreover, although women in the experimental group received more of virtually every type of service than women in the control group, the differences were often relatively small, particularly with regard to education. Given the high level of service receipt by members of the control group, this evaluation should be viewed as a test of New Chance services in comparison to other services available to the New Chance target group, rather than as a test of the value of New Chance services per se.

The results of the New Chance impact analyses were, at this early point, mixed. Participants were largely drawn to New Chance by the prospect of earning a GED (General Educational Development) certificate,[4] and women in the experimental group were more likely than those in the control group to have obtained this credential by 18 months after random assignment (37 percent vs. 21 percent, respectively). The experimental group women were also somewhat more likely to have earned college credits and to be enrolled in college at the follow-up point. However, the two groups were comparable with regard to a number of outcomes for which it had been hoped that the program would produce a positive impact. For example, at the 18-month point, the experimental and control group women were similar with regard to reading skills, depression, stress, drug use, and health. Moreover, women in the experimental group were more likely than women in the control group to have had another pregnancy in the 18 months following random assignment, and were less likely to be using contraception regularly at the time of the 18-month follow-up, although the two research groups had a similar rate of subsequent births at follow-up. It is also worth noting that experimentals were more likely than controls to be living with a partner or husband at follow-up, and that the program's impact on pregnancy occurred only in conjunction with this living arrangement.

As expected, and consistent with their greater investment of time in education and training, women in the experimental group were less likely than those in the control group to have been employed during the first six months after random assignment. Over time the employment rates for the two groups became increasingly similar. During the 18 months of follow-up, the experimental group had worked nine fewer days and had earned $342 less, on average, than the control group. Women in the experimental and control groups had similar rates of welfare receipt over the follow-up period and, at the time of the 18-month interview, the two groups were comparable in terms of employment status and receipt of welfare.

---

[4] This credential is given to those who pass the GED test and is intended to signify knowledge of basic high school subjects.

With respect to outcomes for the children, several impacts were in the hypothesized direction, but were not substantial. For example, children of experimental group members were being raised in homes that were more emotionally supportive than children of control group members, and by mothers who expressed less authoritarian childrearing attitudes; the magnitude of these impacts, however, was small. Overall, the two groups of children were living in home environments that were of similar quality.

Impacts on child care in the follow-up period were, however, sizeable. In particular, children of experimental group members were about twice as likely as children of control group members to have been in a child care center, and were more likely to have entered a regular child care arrangement before age 1.

By and large, the impact results suggest that the effects of New Chance, when they were observed, were fairly pervasive across various subgroups of young mothers. That is, observed differences between the experimental and control groups were generally similar for women of different ethnicities, family backgrounds, etc. With respect to site impacts, no single site stood out as better than other sites across the full range of outcomes considered.

At this point, it is too early to predict the longer-term results for these young mothers and their children. As noted above, previous studies of programs for disadvantaged young mothers have suggested that longer-term outcomes are sometimes more promising than would be anticipated on the basis of short-term findings.[5] A more complete picture of program impacts should emerge when 42-month follow-up data are available. The early findings do, however, indicate that the women in both the experimental and control groups have a considerable way to go before attaining self-sufficiency: At the time of the 18-month interview, 61 percent of the sample had not yet obtained their GEDs or high school diplomas; 65 percent were neither employed nor in an education or training program; and 82 percent were still on welfare.

The New Chance Model and Program Goals

The overall goal of New Chance was to help prepare participants for their dual roles as productive earners and effective parents, while enhancing the cognitive, emotional, and social development of their children. New Chance targeted young mothers aged 16 to 22 who gave birth as teenagers and were receiving welfare. While other initiatives have been directed toward a broad cross section of young mothers on AFDC (including those still in school and high school graduates), New Chance was developed specifically for a highly disadvantaged subgroup of high school dropouts. This target population typically faces a number of formidable barriers to achieving economic self-sufficiency, including low levels of basic skills, a lack of education credentials, limited work experience, the risk of early subsequent childbearing, and a variety of personal and family problems. New Chance was designed as a comprehensive intervention that could address many of these barriers.

---

[5] See, e.g., Polit, Quint, and Riccio, 1988.

New Chance services were defined in guidelines developed by MDRC and were typically offered in two distinct phases. Phase I activities were diverse, but a major focus was on education services, which consisted primarily of instruction in basic academic skills and, for those with better skills, preparation for the GED test. Employability development classes, which were also offered during the first phase, included career exploration and instruction in pre-employment skills (e.g., how to find a job). A variety of personal development services were provided: health education classes and, at some sites, health care services; family planning classes; and life skills education. Case management, including individual counseling, was another feature of the New Chance programs, which were intentionally small in scale and were designed to offer a warm and supportive – but demanding – environment. Most Phase I services were offered at the program site – an integrated "one-stop shopping" approach designed to facilitate participation.

The New Chance model is explicitly two-generational: It seeks to improve the well-being of participants' children as well as that of the mothers themselves. Services designed with the children in mind included parenting education and pediatric health services. Free child care is also part of the New Chance model. The majority of sites operated on-site child care centers, and these centers were encouraged to adhere to guidelines established for New Chance by child development experts.

Phase II activities, which were designed to begin when enrollees had received their GEDs or had been in the program for five months, tended to be more employment-focused and were generally offered off-site. These activities included skills training for specific occupations, paid or unpaid work internships designed to expose participants to work settings, and job placement assistance. While not part of the formal program model, college attendance was a post-GED activity for some of the young women.

The New Chance model was designed to be intensive as well as comprehensive. Program activities were scheduled for 20 to 30 hours per week, and participants were allowed to remain in the program for up to 18 months, with up to an additional year of follow-up case management. As noted earlier, New Chance programs generally enrolled eligible young mothers on a voluntary basis, although in some cases participation could fulfill the requirements of mandatory welfare-to-work programs.

Consistent with the comprehensiveness of the program model, New Chance sought to achieve impacts on a wide variety of outcomes for participating mothers. In the short term, the major objective was to increase the educational attainment of the young mothers, relative to what they might have attained in the absence of the program. Other short-term goals included postponement of further childbearing, good health outcomes, and improved emotional well-being. In the longer term, the program goal was to increase the employment and earnings of participants, and to reduce their receipt of welfare.

New Chance also sought to achieve impacts on participants' children. Through the provision of parenting, health, and child care services, the program aimed to improve the children's cognitive, social, and physical development in the long run. In the shorter term, a major goal was to improve the quality of the parenting and home environments to which the children were exposed, and to provide them with stimulating and developmentally appropriate child care.

It is important to note that some sites have modified the program model over time, largely in response to staff perceptions of enrollees' needs (and mostly after the period studied in this report). Several sites, for example, have added formal linkages with mental health providers to the roster of services prescribed by the program model; they have also come to place a greater emphasis on employment from the outset of a participant's stay.

The Policy Significance of the New Chance Demonstration

The evaluation part of the New Chance Demonstration was designed to answer fundamental questions as to the program's feasibility, effectiveness, and costs. The results are expected to shed light on strategies for promoting the self-sufficiency of women at high risk of long-term welfare receipt. While not directly intended to test proposed changes to welfare legislation, the demonstration will also yield information on issues with which new welfare reform efforts will need to contend. In addition, the evaluation should enhance the understanding of mechanisms for helping out-of-school youth make a successful transition into the labor market.

Because of its two-generational focus, the New Chance Demonstration will contribute to an expanding body of research on programs designed to improve the development and well-being of children being raised in poor families. The demonstration also aims to provide insights into the feasibility and effectiveness of an integrated service model, with integration occurring across both multiple services and two generations.

New Chance was developed and operated during a period of intense concern about the fiscal, social, and personal costs of long-term welfare receipt. The New Chance population is central to the ongoing debate on welfare reform because although teenage mothers account for only about 8 percent of AFDC recipients at any point in time, they typically remain on welfare for many years, and households begun by teenage mothers account for more than half of all welfare expenditures. Young mothers who do not complete high school are especially likely to remain on welfare for long periods.

New Chance began operations during a period when welfare policies were undergoing major changes. New Chance was first implemented in 1989, shortly after the Family Support Act of 1988 was enacted. A key provision of this federal legislation was the creation of the Job Opportunities and Basic Skills Training (JOBS) Program, which gives state welfare agencies increased funding for delivering education, vocational skills training, and other employment-related services to welfare recipients. The JOBS legislation includes a financial incentive structure that encourages states to focus on groups at high risk of long-term welfare receipt, including young parents without a high school diploma. However, few states have moved aggressively to target young mothers with small children (in fact, most New Chance enrollees met the criteria for being required to participate in JOBS, but few were actually required to do so). Moreover, while programs for teenage mothers have proliferated over the past decade, most are for in-school youth, and few have included employment-related services.

The New Chance Demonstration, along with a handful of other demonstrations aimed at teenage mothers, is expected to contribute to knowledge about interventions and policy approaches for families headed by poor young mothers.

The New Chance Evaluation

A rigorous program of research was undertaken to determine New Chance's feasibility, effectiveness (or impacts), and costs relative to benefits. The key features of the research plan for the impact and benefit-cost analyses include the following:

- **Random assignment of a large sample** of eligible applicants to an experimental or a control group.
- **Collection of baseline information** (i.e., information collected just prior to random assignment) on a broad range of sample members' characteristics.
- **Collection of extensive follow-up information** by means of in-home interviews with experimental and control group members and their children at 18 months and 42 months after random assignment.
- **Exhaustive tracing efforts** to locate and interview as many sample members as possible at the two follow-up points, and thereby minimize biases in the findings resulting from loss of sample members.
- **Rigorous statistical procedures** for the analysis of the data.

Random assignment of eligible applicants was the cornerstone of the research design. Properly implemented, this lottery-like procedure ensures that the experimental and control groups do not differ systematically at the outset of a study. During the operational phase of the demonstration, each site recruited about 150 eligible young mothers. Two-thirds were assigned at random to the experimental group and were allowed to participate in New Chance. The remaining one-third were assigned to the control group and were not allowed to participate, although they were free to seek and obtain services through other programs offering similar types of assistance. Because of the initial comparability of the two groups, the control group provides information on what the experiences of the women in the experimental group would have been in the absence of New Chance. Thus, a comparison of the two groups over time yields an estimate of the *net* impacts of the New Chance program, i.e., over and above the effects of the services that controls received.

To assess the impacts of New Chance, follow-up information was to be obtained through in-home survey interviews with sample members at approximately 18 and 42 months after each was randomly assigned to the experimental or control group. The 18-month point was chosen for the first round of interviews because enrollees were allowed to remain in New Chance for up to 18 months. The 18-month survey was designed to measure the short-term effects of the program, with particular emphasis on the mothers' education, fertility, parenting, and use of child care. Over 90 percent of the young women who were randomly assigned (2,106 of the 2,322) were interviewed at the 18-month point, and the surveys of 2,088 of these 2,106 respondents were complete enough to be used in the analysis. Thus, 2,088 women constituted the sample for most of this report's impact analyses, and their interviews were the primary data source used in those analyses.

The longer-term effects of the program, with emphasis on the mothers' economic well-being and their children's development, are being measured through interviews and assessments completed approximately three and a half years (42 months) after random assignment. Additional sources of information for the evaluation include data on program participation from the program's Management Information System (MIS), observations of program operations and child care center functioning, and interviews with program staff.

Finally, insights into sample members' behavior were provided by program staff and by a monograph based on in-depth interviews with 50 former New Chance enrollees conducted an average of 30 months after the young women left the program (Quint and Musick, 1994). These qualitative data indicate the complexities of the young women's lives and the many interrelated factors making for progress toward economic independence or slowing movement toward that goal. To convey a sense of these complexities and to illuminate certain issues raised by the implementation and impact findings, examples drawn from the monograph appear in boxes in this Executive Summary.

The New Chance Sample

A threshold question for the research was whether it would be feasible to recruit eligible young mothers to enroll voluntarily in a comprehensive and intensive program such as New Chance. Recruitment required ongoing effort and began at a point when many sites were still developing the program infrastructure. Nonetheless, most programs were able to reach the demonstration's recruitment goal of at least 150 applicants per site.

- **The New Chance programs succeeded in recruiting young mothers with educational and other barriers to employment.**

Although there was diversity among the program applicants, on the whole, they can be characterized as a highly disadvantaged group of young women.[6] As shown in Table 2, they averaged just under 19 years of age and, on average, had first given birth before they had reached the age of 17. Most (77 percent) were members of minority groups, and fewer than one in 10 had ever been married. About one-third already had two or more children, and the majority had a child who was younger than age 2. Indicative of their disaffection from school, 37 percent had dropped out before

---

[6] New Chance targeted young mothers who did not have a high school diploma or GED and were receiving welfare. However, it was recognized that many needy young women would not meet all the eligibility criteria, so each site was allowed to accept for random assignment some applicants (up to 25 percent of all those it accepted) who were high school graduates but had low levels of reading skills and who were economically disadvantaged but not on AFDC. Only 11 percent of the entire research sample were admitted under this "window," however.

TABLE 2

SELECTED CHARACTERISTICS OF THE NEW CHANCE SAMPLE AT RANDOM ASSIGNMENT

Characteristic	Average or Percent
Average age (years)	18.8
Average age at first child's birth (years)	16.8
Ethnicity (%)	
Black, non-Hispanic	52.4
Hispanic	22.3
White	22.7
Other	2.5
Ever married (%)	9.9
Living with mother (%)	34.4
Had more than 1 child (%)	35.1
Average number of pregnancies	1.9
Average age of youngest child (years)	1.2
Received high school diploma or GED (%)	6.3
Average highest grade completed	9.9
Average number of years since last attended school	2.4
Average reading level (grade equivalent)	8.4
Had no employment experience (%)	21.5
Did not work in prior 12 months (%)	63.3
Receiving AFDC on own or other's grant (%)	94.8
Family was ever on AFDC when applicant was growing up (%)	63.8
CES-D (depression) Scale (a) (%)	
0-15 (not at risk)	47.0
16-23 (at some risk)	25.9
24-60 (at high risk)	27.2
Sample size	2,088

NOTES: Distributions may not total 100.0 percent because of rounding.
(a) The Center for Epidemiological Studies Depression (CES-D) Scale is a widely used measure of depression; scores can range from zero to 60.

Lives in Flux

Profiles from the New Chance monograph (Quint and Musick, 1994) illustrate the diversity of program participants and of their post-program courses of action. The data also suggest that progress toward self-sufficiency was rarely smooth and uninterrupted, even among young women who were relatively successful in New Chance and earned a GED while in the program. How much progress a young woman made in part reflected such personal qualities as resilience, motivation, and personableness; but the fluctuating, up-and-down nature of their lives is also attributable in part to the difficult, sometimes unpredictable circumstances in which they lived, the institutions with which they interacted, and the actions of other people who were central to their lives. (The young women described here have been given pseudonyms, and certain identifying details have been changed.)

Jodie has displayed considerable persistence in the face of limited academic proficiency (she read at only the 7.0-grade level when she entered New Chance) and other difficulties. She took 15 months to get her GED: After failing the test the first time, she grew discouraged and took a four-month leave of absence from the program, but when she rejoined, she did well, and earned the certificate four months later, when she was eight months pregnant with her third child. Like her two sisters, who were both nurse's aides (but who subsequently became addicted to drugs), Jodie entered a nurse's aide training program. She particularly liked working with old people in her clinical internship at Mountainside Nursing Home, even though she had to get up at 5 A.M. so that she could deliver her children to her mother's place before taking the 40-minute bus ride to Mountainside. At the end of the program, she took the test for certification, passing the written part but not the clinical part; she was too nervous, she explains, to demonstrate the various techniques correctly. She took that part again a few months later, passed it, and at the time of the interview, had been working at Mountainside for eight months, earning $7.20 an hour, and was off welfare. (She would prefer a job closer to home, but hasn't found one, and won't quit Mountainside until she has another position lined up.) Jodie managed to stay on track despite the fact that the father of her third child – they were formally engaged and had gone so far as to get the required blood test – got into a fight and had to flee the city for his life. At the time of the interview, Jodie had to cope with another problem: Her mother, a dependable source of free child care, had grown tired of the crime-ridden environment and had decided to move back to the small town where she was born. When interviewed, Jodie had yet to find the low-cost child care for her three children that she will need in order to continue working.

Immediately after earning her GED, **Mercedes** enrolled in a community college as a human services major but found one of her academic courses dauntingly hard. Hungry for male affection (her father, she recalls, often beat her with a strap, and her relationship with her stepfather is a difficult one), she began an affair with a married man and soon became pregnant with her third child. Driving home intoxicated from a party, she was in a serious automobile accident from which it took weeks to recover, and she dropped out of college. Her baby, born eight months later, died within hours after birth, and Mercedes was depressed for some time. When she came out of her depression, she decided to enroll in a training program to become an accountant but because she had dropped out of school, the local JTPA coordinating agency refused to pay the cost of child care for her children. Eventually, the JTPA coordinator relented, and Mercedes was able to enroll in the program. She earned A's

(continued)

and B's there and was due to receive her training certificate two months after the interview. Mercedes still goes out drinking a couple of weekends a month, but she is determined to find a job and make something of herself; she is happy that her children see that she is going to school "so they know that life isn't just sitting on your butt collecting welfare." She has also decided that two children are enough, and her contraceptive practice is firmly under control. Her New Chance case manager says of Mercedes: "She has turned her life around."

After receiving her GED, **Yvonne**, who wants to become an RN, held a New Chance-arranged work internship in a nursing home. She quit after about two months, citing her distress at seeing the nurse's aides and LPNs speaking roughly to the patients and even hitting them. At the time of the interview, she had been at home for more than a year and a half, except for a brief stint at a telemarketing job. Her explanation is that she wants to wait until her daughter, now four, is old enough for a full-day kindergarten. She claims there is no day care center – and for that matter, no one except her mother – that she trusts to care for her daughter. She also wants to shelter her child from the drugs and violence and "negative people" who live all around her, feeling that if she raises her right at this juncture, her daughter won't, like so many other children, end up in the streets, using drugs, or dead. Once her child is in kindergarten, Yvonne plans to enter a training program. She figures that without additional training, it won't be worth her while to work, because the rent in her subsidized apartment will increase to reflect her earnings, and she will lose Medicaid coverage. Time will be the test of whether Yvonne will translate into action her expressed intention of rejoining the world of work.

Anita registered low self-esteem and was quite depressed when she entered New Chance – feelings she may have concealed under the feisty, tough persona she presented to the world. (Her case manager described Anita as "crude, rude, and sharp" – one of the few young women on her caseload with whom the case manager had difficulty forming a close relationship.) Nonetheless, no one in Anita's family received welfare, and Anita seemed intent on becoming self-sufficient as well. After getting her GED, Anita enrolled in a skills training program. Five months later, a teacher with whom Anita didn't get along (and toward whom she was verbally abusive) accused her of having cheated. Anita hotly denied that she had cheated and left the program. She enrolled almost immediately in a proprietary college notorious for its exploitative practices and also became pregnant with her second child. Anita attended a program to become a legal secretary until her eighth month of pregnancy; but when she tried to re-enroll the next semester, after her daughter's birth, she was told that her student loan had gone into default and that she would have to pay $500 in order to return. Out of school for a year and a half at the time of the interview, Anita believes her outstanding loan will prevent her from being admitted to another training program. (She is apparently mistaken in this belief, but she has severed her ties with New Chance and seems to have no one else who could set her straight on this issue.) She speaks of getting a job but has done little to find one. Part of the reason may be that she broke up with her live-in boyfriend and has no one to take care of her children while she looks for work.

(continued)

Lives in Flux (continued)

Julia, a bright and articulate young woman, did so well in New Chance that the program coordinator at her site often asked her to help with recruitment by talking about her experiences in the program before groups of young mothers at local welfare offices. After receiving her GED, she enrolled in a community college. Her choice of accounting as a major seems both realistic and muddled (and clearly illustrates the need for better vocational guidance): She reasoned that the occupation was a stable one ("everybody could always use an accountant") and saw it as a good way to improve her skills ("I was never good with numbers and math and adding and everything, and I thought, well, this is a good opportunity for me to get better at my math"). However, at the very beginning of her first term, her daughter became very sick and had to be hospitalized for over a week. Spending all her time at the hospital, Julia fell behind in her classes and decided to take the semester off. She ended up essentially spending the next year at home with her daughter, and at the time she was interviewed, she was uncertain whether she would return to school, get a job, or both. Along with her daughter's illness, an unintended pregnancy and a passionate romance consumed her energy and disrupted her progress. She opted to give the baby up for adoption, an act that she felt was right but caused her considerable pain and estrangement from her mother, who wanted her to keep the child. Shortly thereafter, she became deeply involved with a man with whom she lived for a while and who she hoped would marry her, but who turned out to be abusive (and possibly involved in illegal activities as well). On the positive side, Julia appears to be a loving and concerned mother; the shabbily furnished room in which she lives has a number of children's books, and the recognition that her fierce fights with her boyfriend were terrifying to the little girl was one factor that led her to end the relationship.

Edna, the mother of a five-year-old, started receiving AFDC when she was pregnant and has been on the welfare rolls for six years. For the last couple of years, she has been using public assistance as a vehicle for moving forward and for investing in herself: after receiving her GED, she enrolled in a two-year business skills training program offered by a community college, attending during the summers as well as the school year. She studies several hours a day and has earned a 3.85 grade average, although her efforts have been accompanied by isolation from friends and stress-induced illnesses. Edna's mother, while helping out by caring for her granddaughter while Edna is in school, provides little consistent psychological support, often telling Edna that all her efforts will go for naught. On the other hand, her sister has been a source of unswerving emotional, financial, and practical assistance. For four years, Edna has been in an ambivalence-filled relationship with Pete: Although he is resentful of the time she spends on schoolwork and fearful that she will leave him for someone more successful, she thinks of herself as a "one-man woman." She practices contraception faithfully and is determined to persevere in her studies, get a job as an administrative assistant, and once she is working, continue attending college part-time for an associate's degree, and ultimately, become a paralegal. The steadfastness of purpose that characterizes her present activity stems from the time after her daughter was born when, abandoned by the child's father, Edna entered a period of deep depression: "I was real depressed for a long time, for a lot of years. All those years that I didn't go to school, I was really depressed. I was just in my room, I didn't know what to do. So that's why I have all this determination to do something, because I don't want to ever feel that way again."

their first pregnancy. Applicants had typically been out of school for more than two years and had reading skills just above the 8.0-grade level. While most had some work experience, few had worked full-time over a sustained period (not shown in the table); the majority had not worked at all in the 12 months prior to applying to the program. Most (64 percent) had been raised in families that had received welfare at some point in their childhood. While feelings of depression tend to fluctuate over time, it is noteworthy that at the time they applied to New Chance, 53 percent of the young mothers – more than twice the proportion in the general population – had a score on a widely used scale that indicates an above-average risk of clinical depression.

In short, the young women applying to New Chance had characteristics that, for many, would pose a challenge for speedy progress toward a GED. They typically had been out of school for lengthy periods, and had low levels of basic skills. They were also mostly adolescents, not yet fully mature nor certain of their goals. And they were responsible for the care of very young children.

Findings on the Implementation and Costs of New Chance

The feasibility of mounting a complex and comprehensive program like New Chance within a variety of geographical and administrative contexts was a key question for the evaluation, because of its relevance to possible replication of the model and its importance to an understanding of program impacts: If the program was not adequately implemented, then the impact analysis might not be a fair test of what the model could achieve. As indicated in Table 1, the backgrounds of the New Chance sites were diverse; furthermore, no site had previously offered all the services covered by the program model. Thus, the ability of sites to implement the model could not be taken for granted.

- **The 16 demonstration sites put in place all the Phase I components; with only a few exceptions, the sites were able to offer the required hours of each service that were prescribed by the program guidelines.**

Despite the different service traditions of the sponsor agencies – and despite a start-up period of only about six to eight months – the sites were all able to mount the Phase I components of the New Chance model and to deliver a treatment that was reasonably uniform. However, building the program infrastructure required considerable and ongoing effort; the programs have continued to evolve and mature over time.

At all sites, education was a central activity during Phase I, usually scheduled for about 12 to 15 hours per week. Parenting and life skills classes were each scheduled for about 2 hours weekly during the first phase. Education proved to be one of the easiest components to implement, in part because most sites had prior experience in offering education classes and in part because enrollees were themselves interested in getting their GED certificate.

Employability development services and individual counseling relating to family planning posed the greatest challenges. Case managers, because of personal discomfort, lack of expertise, or time constraints, did not consistently follow up on the young women's family planning practices in individual counseling sessions. The implementation of such activities as career exploration and pre-employment skills instruction was hampered by such factors as time constraints and the lack of suitable curricular materials.

- **The quality of child care at the on-site day care centers was generally congruent with child care experts' guidelines; moreover, the care was of higher quality than that typically provided by child care centers serving primarily low-income families.**

Regular child care was provided to New Chance participants at 9 of the 16 sites, and two additional sites offered child care on a temporary, drop-in basis. Programs without on-site facilities helped participants with their child care arrangements, often through linkages with nearby child care centers. Based on questionnaires completed by staff at the on-site centers, it was determined that the New Chance child care centers generally met or exceeded experts' standards in terms of structural characteristics such as group size and child-to-staff ratios. Additionally, observers who were trained to rate various aspects of child care visited 11 centers (including four off-site centers) and found that they were providing good-quality care that compared favorably to the care provided in centers serving similar families, based on data from two major studies of child care centers.

- **Phase II activities proved more difficult to implement, and were less uniform across sites, than the Phase I components.**

Phase II activities, mostly delivered off-site, required considerable individualized attention. Staff had to find an appropriate activity from those available in the community to meet participants' needs, interests, and abilities, and in many cases this was not an easy task. Phase II activities typically required New Chance staff to coordinate with outside agencies (for skills training, work internships, or college enrollment), and these agencies often had few or no mechanisms for addressing the complex needs and problems of this population. Moreover, because of the demands of their on-site caseloads, case managers were seldom able to maintain the biweekly contact with off-site participants that was specified in the program guidelines.

- **Sponsor agencies spent an average of $5,073 per experimental, excluding child care costs, operating New Chance. Child care costs amounted to an additional $2,573 per experimental.**

Excluding child care costs, sponsor agencies spent an average of $5,073 per experimental on New Chance. Again excluding child care costs, other agencies that provided or helped to provide services to New Chance participants spent $1,380 per experimental. Total child care costs from both kinds of agencies averaged $2,573 per experimental. Thus, the total sponsor and other agency cost of New Chance averaged $9,026 per experimental; it ranged from $4,758 to $16,846, depending on the site. Nearly three-quarters of New Chance's total cost was attributable to child care, case management, and education.

These estimates represent the *gross* costs of the program. The *net* cost of New Chance – the difference between what was spent on experimentals and what was spent on controls, who received extensive services on their own – will be presented in the benefit-cost analysis in the final New Chance report.

Findings on Participation

The participation findings indicate that while many enrollees – especially GED recipients – received the comprehensive and intensive treatment that was intended, the majority of all enrollees received a less intensive, less employment-oriented set of services.

- **The majority of enrollees (89 percent) took part in one or more Phase I activities. Sixty-five percent of GED earners participated in skills training or a work internship, the principal Phase II activities, but only 25 percent of the non-GED earners did so.**

About 89 percent of the enrollees participated in at least one program activity. On average (including zero hours for the 11 percent who did not participate at all), the young women participated in New Chance activities for about 300 hours. One-fourth of all enrollees had between one and 100 hours of activity, while nearly another fourth had more than 500 hours.

The program guidelines stated that participants should move into Phase II activities after receiving their GED certificate, or by the fifth month in the program if the GED had not been obtained. In practice, program staff principally placed GED completers in Phase II activities, and almost two-thirds of those who earned a GED participated in skills training or a work internship. Young women who were still enrolled at the fifth month but had not yet passed the GED test were encouraged to continue working toward that goal, rather than to move into a Phase II component. In addition, many young women, especially those experiencing little academic success, left the program early. As a consequence, only 25 percent of those without a GED by the 18-month point had participated in skills training or a work internship.

Most young women rated their experience in New Chance favorably and reported especially liking the program staff; the caring, support, and individual attention they received; the other students; and the opportunity the program afforded to meet new students. Nonetheless, absenteeism was a common problem at most sites; MDRC staff who monitored program operations noted that, on a typical day, only half of those enrolled were actually present. In general, women who were initially the most disadvantaged, educationally and otherwise, tended to have fewer hours of participation. Reasons for absenteeism cited by the young women included: their own illnesses (including pregnancy-related discomforts) and those of their children, disruptions in child care arrangements, conflicting appointments, and lack of support or active discouragement from family members or boyfriends.

The typical enrollee was active in the program for about six months, with the months of activity not necessarily being continuous: Periods of program activity were sometimes interspersed with periods of inactivity.

- **Women in the experimental group received more services than women in the control group during the 18 months of follow-up. However, a very high percentage of the control group had participated in various activities, especially education programs.**

As a voluntary program, New Chance attracted young mothers who were presumably motivated (at least at the time they applied to New Chance) to receive the services the program offered. Thus, many of the women who were randomly assigned to the control group (and who therefore could not participate in New Chance activities) were likely to go elsewhere in their communities for education, skills training, and other services. The impact analysis examined whether there were *differences* in participation levels between the two research groups.

Table 3 shows the experimental-control differences in the percentage of young mothers who participated in various activities through 18 months of follow-up and, in the right-hand panel, the average number of weeks of participation in education and employment-related services. The majority of women in both groups participated in some type of education program after random assignment, but experimentals (85 percent) were more likely than controls (60 percent) to do so. Moreover, experimentals averaged nearly twice as many weeks in education programs as controls (26 versus 14 weeks, respectively), indicating that experimental group members who received education services participated longer, on average, than did control group members.

Experimental-control group differences were especially large with regard to parenting and other personal development services. For example, more than three times as many experimentals as controls participated in parenting classes, family planning classes, and health education during the 18 months of follow-up. These participation patterns probably reflect the fact that many of the young mothers in both research groups were primarily seeking education services when they applied to New Chance. Apparently, control group members did not pursue personal development services on their own, and such services may not have been offered at the programs where they obtained education.

Overall, the experimental-control group differences were consistent across a variety of services, but the differences were most pronounced during the first few months after random assignment and diminished over time. At the 18-month point (not shown in the table), the two groups were equally likely to be participating in various education or employment programs, with one exception: More experimentals (5 percent) than controls (3 percent) were attending college at follow-up.

Impacts on Educational Attainment and Achievement

The most important short-term goal of New Chance, and of the participants themselves, was the improvement of their education credentials.

- **By the 18-month point, a higher percentage of women in the experimental group than in the control group had obtained a GED certificate, and a higher percentage had earned credits toward a college degree.**

Table 4 shows that 37 percent of the experimentals versus 21 percent of the controls had earned a GED certificate by the time of the 18-month follow-up. Experimentals were actually less likely to have received a high school diploma than controls, reflecting the fact that New Chance did not offer high school classes, while a small minority of controls returned to high school. Taken together, a higher proportion of experimentals (43 percent) than controls (30 percent) achieved a diploma or GED.

New Chance's positive effect on attainment of a GED was statistically significant for virtually all subgroups of women (not shown in the table). A major exception was women who read below the

TABLE 3

IMPACTS OF NEW CHANCE ON SERVICES RECEIVED WITHIN 18 MONTHS AFTER RANDOM ASSIGNMENT

	Ever Participated in Activity (%)			Average Weeks of Participation (b)		
Activity (a)	Experimentals	Controls	Difference	Experimentals	Controls	Difference
Any education program	85.3	60.4	24.9 ***	25.7	13.9	11.8 ***
Basic education/GED	79.4	47.1	32.4 ***	20.7	8.7	12.0 ***
High school	2.5	3.6	-1.1	0.6	0.8	-0.2
College	12.5	7.9	4.6 ***	3.3	2.1	1.1 **
Other education (c)	20.1	17.8	2.3	5.2	4.9	0.3
Skills training/unpaid work	35.2	23.3	11.8 ***	8.2	5.4	2.8 ***
Skills training	33.3	22.5	10.8 ***	7.6	5.1	2.5 ***
Unpaid work	6.3	2.2	4.1 ***	0.6	0.2	0.3 **
Other services						
Parenting classes	66.5	20.6	45.9 ***	N/A	N/A	N/A
Family planning classes	51.7	11.9	39.8 ***	N/A	N/A	N/A
Health education classes	49.3	11.0	38.3 ***	N/A	N/A	N/A
Personal counseling	40.9	14.6	26.4 ***	N/A	N/A	N/A
Job counseling	53.6	19.4	34.2 ***	N/A	N/A	N/A
Life skills classes	51.6	12.4	39.2 ***	N/A	N/A	N/A
Sample size	1,408	680		1,408	680	

NOTES: Calculations for this table used data for all sample members, including those who did not participate in the activity and experimentals who did not participate in New Chance. Rounding may cause slight discrepancies in sums and differences.

Statistical significance levels are indicated as *** = 1 percent; ** = 5 percent; * = 10 percent.

(a) The services listed, with the exception of high school classes, are major components of the New Chance model. For controls, services were obtained at programs or agencies other than New Chance. For experimentals, the services were obtained either at New Chance or, if they were served by additional programs, elsewhere.

(b) N/A indicates that data were not available.

(c) Includes proprietary schools.

TABLE 4

IMPACTS OF NEW CHANCE ON EDUCATIONAL ATTAINMENT AND ACHIEVEMENT AT 18 MONTHS AFTER RANDOM ASSIGNMENT

Outcome	Experimentals	Controls	Difference
Education credential at 18 months (a) (%)			
GED or high school diploma	43.1	30.0	13.1 ***
GED	36.8	21.1	15.8 ***
High school diploma	6.6	9.2	-2.6 ***
Trade certificate or license	12.5	12.4	0.1
Credits toward A.A. or B.A. degree	9.8	7.1	2.6 **
Average TABE reading score at 18 months (b, c)	748.7	748.3	0.4
Distribution of reading levels at follow-up (c) (%)			
7th grade or below	41.4	41.7	-0.4
8th or 9th grade	30.6	28.5	2.1
10th or 11th grade	9.6	11.9	-2.3
12th grade or above	18.4	17.9	0.5
Sample size	1,408	680	

NOTES: Calculations included data for sample members who had values of zero for outcomes and for experimentals who did not participate in New Chance. Rounding may cause slight discrepancies in sums and differences.

Statistical significance levels are indicated as *** = 1 percent; ** = 5 percent; * = 10 percent.

(a) The percentages shown are for all sample members, including the 6 percent who had already achieved a high school diploma or GED when they applied to the program.

(b) The test administered was the Tests of Adult Basic Education (TABE), Survey Form, a 30-item test of reading vocabulary and reading comprehension. The scores shown are equivalent to a 7.8-grade reading level.

(c) The sample size for the TABE was 1,374 experimentals and 672 controls.

sixth-grade level when they applied to the program: About 20 percent of these women – among both experimentals and controls – received a GED or high school diploma.

There were no group differences with respect to receipt of a trade certificate or license at follow-up. However, consistent with their higher rates of enrollment in college programs, experimentals were more likely than controls to have accrued credits toward a college degree.

- **The experimental and control groups had similar average scores on a test of literacy administered at the 18-month interview.**

As shown in Table 4, the scores on a brief literacy test administered at the 18-month interview were virtually identical for experimentals and controls – scores that represent reading skills at the 7.8-grade level. About 40 percent of each group had reading scores at or below the seventh-grade level at follow-up. Thus, the program's impacts on attainment of an education credential were not matched by impacts on educational achievement (specifically, reading scores), a discrepancy that possibly reflects differences in what the GED test and the literacy test measure.

Impacts on Fertility and Living Arrangements

New Chance sought to improve the young mothers' prospects for long-term self-sufficiency by encouraging them to postpone subsequent pregnancies. The program model included instruction on contraceptive options, linkages with family planning providers, and life skills classes that covered decision-making relating to sexuality and reproduction.

- **Women in the experimental and control groups had comparably high rates of births during the follow-up period. However, the experimental group reported a higher rate of pregnancies and a higher rate of abortions.**

Although only 18 months had elapsed between random assignment and the follow-up interview (and despite the fact that women who were pregnant when they applied were not eligible for New Chance), more than one out of four young mothers in both the experimental and control groups had had another baby during the follow-up period (see Table 5). Moreover, more than half of each group of women had gotten pregnant again since random assignment, with a higher rate of pregnancy among the experimentals than among the controls. The vast majority of the post-random assignment pregnancies in this sample (87 percent) were reported to have been unplanned. A minority of pregnancies in the two groups were reported to have been terminated by abortions, but the rate was higher among the experimentals.

Women in the experimental group were actually somewhat more likely than controls to have planned a pregnancy. Moreover, they were more likely to say that they expected that they would have another child within two to four years. At the time of the follow-up interview, more experimentals than controls were at risk of another pregnancy because they were not using contraception regularly.

The findings with regard to subsequent pregnancy, while inconsistent with the intent of New Chance, are consistent with findings from other evaluations. As noted above, most programs for

TABLE 5

IMPACTS OF NEW CHANCE ON FERTILITY-RELATED OUTCOMES THROUGH 18 MONTHS AFTER RANDOM ASSIGNMENT

Outcome	Experimentals (%)	Controls (%)	Difference
Had one or more post-random assignment			
Pregnancy (a)	57.0	53.0	4.0 *
Planned pregnancy	8.0	6.1	1.9
Unplanned pregnancy	49.5	47.8	1.7
Birth	28.4	26.2	2.2
Abortion	14.9	11.1	3.8 **
Miscarriage	8.4	9.5	-1.1
Future childbearing expectations			
Expects to have no more children	53.5	56.1	-2.6
Expects to have another child within next 12 months	7.2	9.1	-1.9
Expects to have another child in 13-48 months	21.5	15.7	5.8 **
Expects to have another child in 49 months or more	17.8	19.1	-1.3
Birth control status at follow-up			
Sexually abstinent, not pregnant	17.8	19.5	-1.7
Sexually active, contracepting regularly	37.0	41.0	-4.0 *
Sexually active, not contracepting regularly	30.2	25.2	4.9 **
Pregnant	15.0	14.3	0.8
Sample size (b)	1,366	658	

NOTES: Calculations included data for sample members who had values of zero for outcomes and for experimentals who did not participate in New Chance. Rounding may cause slight discrepancies in sums and differences.

Statistical significance levels are indicated as *** = 1 percent; ** = 5 percent; * = 10 percent.

(a) Because some women had more than one subsequent pregnancy, the percentages with a planned and unplanned pregnancy do not total the percentage with any subsequent pregnancy.

(b) A slightly smaller number of experimentals and controls responded to fertility-related questions, resulting in a total sample size of 2,024. The question on future childbearing expectations was asked of half the research sample, selected at random.

disadvantaged young mothers have found that it is extremely difficult to reduce the rate of subsequent pregnancies and births among women who gave birth as teenagers; in several such programs, births to program participants have actually exceeded those to members of a control or comparison group.

- **Women in the experimental group were more likely than those in the control group to be living with a partner or husband at follow-up, while women in the control group were more likely than women in the experimental group to be living with a parent or grandparent.**

At the time of the follow-up interview, when these young women averaged about 20 years of age, 35 percent of the controls, compared to 28 percent of the experimentals, were living in a household that included a parent or grandparent. In contrast, more experimentals (23 percent) than controls (20 percent) were living with a partner or husband with no parent present; approximately 8 percent of both experimentals and controls were married at the 18-month point. Thirty-six percent of the experimentals and 34 percent of the controls were living alone with their children, without any other adult. Affecting participants' living arrangements was not a specific objective of New Chance, but the impact possibly reflects the program's assistance with housing for participants whose living arrangements were in crisis.

Women who were living with a partner or husband at follow-up were more likely than women in other living arrangements to have had a pregnancy and birth since random assignment (and were less likely to be contracepting regularly). However, they were also more likely to be employed at the time of the 18-month interview, and considerably less likely to be receiving welfare.

- **The program's impact on subsequent pregnancy occurred only in conjunction with coresidence with a partner or husband at the time of the 18-month follow-up.**

Factors Influencing Contraceptive Behavior: One Young Woman's Story

At the time she was interviewed for the New Chance monograph (Quint and Musick, 1994), **Virginia** was not using contraception of any kind. Her story illustrates the many factors that could contribute to a young woman's becoming pregnant again, including the contraceptive ignorance that persisted despite extensive efforts on the part of the New Chance programs to provide detailed family planning information.

Despite the fact that Virginia already had had one post-GED pregnancy that resulted in a therapeutic abortion, she was convinced that her body would react badly to birth control pills, and she was unfamiliar with or distrustful of other contraceptive methods. Moreover, her boyfriend wanted her to have a child by him: "He wants a baby bad, you know, 'cause, like, he always say all his friends got kids and he don't." She also had no immediate career goals that would provide the impetus to delay childbearing. Finally, she did not believe that a new baby would interfere with her life plans; her nieces or her friends, she was sure, would be willing to babysit for her: "So I don't think one more child would have interfered."

When living arrangements and subsequent pregnancy were considered jointly, the experimental group differed from the control group only with respect to *both* living with a partner *and* having had a pregnancy since random assignment – a situation that was true for 16 percent of the experimentals and 13 percent of the controls. Comparable percentages of women in both groups had gotten pregnant but were *not* living with a partner or husband at follow-up (40 percent of each group).

- **The program's impact on GED attainment was sustained even among those with a post-random assignment pregnancy.**

A higher percentage of women in the experimental group (21 percent) than in the control group (13 percent) obtained a GED certificate *despite* having had a pregnancy during the follow-up period. In contrast, controls (40 percent) were more likely than experimentals (35 percent) to have had a pregnancy *without* achieving their GED.

Impacts on Women's Health and Emotional Well-Being

New Chance aimed to improve the health practices of participants and to facilitate access to appropriate health care services. Through its supportive and personalized environment, New Chance also sought to foster positive emotional growth among participants.

- **With respect to the measured health outcomes for the mothers, there were no program effects.**

The 18-month survey included a limited number of questions regarding the mothers' health and health care. There were no differences between the two research groups on any of these measures. For example, women in the experimental and control groups were equally likely to rate their health as very good or excellent; to have had sick days in bed and to have been hospitalized since random assignment (other than for childbirth); to have had a sexually transmitted disease in the previous year; to have used drugs in the month prior to the interview; and to have gotten high on alcohol at least once in the prior month. (In both research groups, only a minority of young mothers – 14 percent of the experimentals and 13 percent of the controls – said that they had used drugs the previous month, but 37 percent of the experimentals and 39 percent of the controls reported having gotten high on alcohol; neither of these differences was statistically significant.) The groups were also similar with respect to health care coverage at follow-up.

- **Levels of depression and stress were comparable among the experimental and control group women at follow-up. However, experimental group women were at an advantage with respect to two indicators of social support.**

Although both groups had more favorable scores on the depression scale at follow-up than at random assignment, nearly half of the women (45 percent of the experimental group and 44 percent of the control group) obtained a score indicative of being at risk of clinical depression. Controls actually had slightly more improved scores on the depression scale than experimentals. Experimentals were, however, less likely than controls to report that they had no one to turn to for emotional support. Moreover, when asked to rate their satisfaction with available social support, experimentals gave a higher rating than controls. The two groups were comparable on a scale that measures daily stress and on a scale that measures perceived control over life events. Overall, then, program impacts on indicators of emotional well-being were mixed, and the observed experimental-control group differences tended to be small.

Impacts Relating to Children

At the 18-month interview, impacts relating to sample members' children were measured in three areas: parenting attitudes and the quality of the home environment, child care, and health and health care.[7]

- **The home environments of children of experimental and control group members were largely similar, but children of experimental group members were living in home environments that were more emotionally supportive. Mothers in the experimental group also reported less authoritarian childrearing attitudes.**

At the 18-month interview, the overall quality of the children's home environment was measured with a short form of the widely used Home Observation for Measurement of the Environment (HOME) Scale. Scores on the HOME's Emotional Support subscale – a subscale that measures the degree to which the mother's interactions with her child are characterized by warmth and supportiveness – were more favorable for the experimental group than for the control group. The impact on this subscale is noteworthy because the measure is based almost entirely on interviewer observations of mother-child interactions rather than on the mothers' own reports and because, unlike some of the other subscales, a family's material well-being did not affect the score it received. However, the magnitude of the impact on the Emotional Support subscale was small. Moreover, there were no group differences on the total HOME scale, nor on subscales that measure cognitive stimulation, the quality of the physical environment, or the level of harsh discipline in the home.

The 18-month interviews also included three brief parenting scales. Experimental and control group mothers had similar scores on the Maternal Warmth/Responsiveness and Parenting Stress scales. However, the experimentals had lower average scores than controls on the Maternal Control/Punitiveness Scale – a scale designed to tap the mother's authoritarian (versus a more democratic or permissive) style of raising and disciplining a child. Overall, then, New Chance's impacts on parenting and the home environment were modest, but all observed group differences were in a positive direction.

- **Children of experimental group members were more likely than children of control group members to have been in a non-maternal child care arrangement after random assignment, and were especially more likely to have used center-based child care. Children of experimental group members were also more likely to have been in a regular child care arrangement prior to age 1.**

---

[7] These data were gathered only for one child of each sample member, selected at random from the children she had at the time of random assignment. However, as noted in Table 2, about two-thirds of the sample had only one child at that point.

The majority of control group members' children (85 percent) had spent some time in a regular child care arrangement since random assignment, but experimental group members' children were even more likely to have done so (95 percent). Moreover, experimentals' children had spent more time during the follow-up period in child care and were more likely to have entered child care before their first birthday. Control group mothers were most likely to have used child care provided by a grandparent (43 percent), and experimental group mothers were equally likely to have used this type of arrangement. However, nearly twice as many experimentals (63 percent) as controls (33 percent) had placed their child in a child care center during the follow-up period. Over half of all experimental group mothers had used child care that was directly provided on-site by the New Chance programs.

Differences between the groups with respect to child care patterns diminished over time. About half of both experimentals' and controls' children were in child care at follow-up, and the two groups used similar types of arrangements at that point. Nevertheless, children in the two groups were clearly exposed to different types of child care during the year and a half after random assignment, and these experiences may have effects on the children's later developmental status.

- **Children's health outcomes were mostly comparable in the two groups.**

Based on the limited number of questions on children's health outcomes that were included in the 18-month survey, the two groups of children appeared to be similar at follow-up. Children of experimental and control group members were comparable with respect to maternal ratings of their overall health, number of sick days in bed and incidence of hospitalizations since random assignment, incidence of injuries and accidents during the follow-up period, and health care coverage. However, while the vast majority of mothers said that they had a particular doctor or clinic for their children, experimentals were more likely than controls to give that response.

Impacts on Employment, Earnings, and Welfare Receipt

New Chance was designed with the expectation that higher levels of initial investment in education and employment-related programs by the experimental group would translate into levels of educational attainment higher than the control group's and, eventually (although not necessarily at the 18-month point), into better labor market outcomes and lower rates of welfare receipt.

- **Control group women were more likely than experimental group women to have been employed in the first few months after random assignment, but employment rates for the two groups converged over time. More than 40 percent of each group had been employed at some point during the follow-up period.**

As indicated in Table 6, small percentages of the two groups had worked in the first two quarters after random assignment, but a higher percentage of controls had done so. This pattern is consistent with the fact that women in the experimental group initially spent more time in human capital development activities (education and training) that interfered with labor force participation, resulting in some short-term employment losses relative to the control group. This phenomenon is frequently observed in programs for disadvantaged groups. However, the reduction in employment was relatively small: On average, controls worked less than two weeks more than experimentals since

TABLE 6

IMPACTS OF NEW CHANCE ON EMPLOYMENT AND WELFARE RECEIPT THROUGH 18 MONTHS AFTER RANDOM ASSIGNMENT

Outcome and Follow-Up Period	Experimentals	Controls	Difference
Ever employed (%)			
Months 1-3	8.0	12.7	-4.7 ***
Months 4-6	14.4	19.2	-4.8 ***
Months 7-9	17.9	20.3	-2.4
Months 10-12	18.5	21.2	-2.7
Months 13-15	22.3	23.8	-1.5
Months 16-18	26.8	26.3	0.4
Months 1-18	42.6	44.9	-2.2
Average number of weeks employed in months 1-18	9.1	10.8	-1.8 **
Average earnings in months 1-18 ($)	1,366	1,708	-342 **
Ever received AFDC (%)			
Months 1-3	95.7	94.8	1.0
Months 4-6	93.9	91.0	2.9 ***
Months 7-9	91.2	89.2	2.0
Months 10-12	89.8	88.7	1.0
Months 13-15	88.3	88.6	-0.3
Months 16-18	88.6	87.6	1.0
Months 1-18	98.0	97.6	0.5
Receiving welfare at 18 months (%)	82.1	81.5	0.7
Average total income in the month prior to the follow-up interview (a) ($)	802	799	3
Sample size	1,408	680	

NOTES: Calculations included data for sample members who had values of zero for outcomes and for experimentals who did not participate in New Chance. Rounding may cause slight discrepancies in sums and differences.

Statistical significance levels are indicated as *** = 1 percent; ** = 5 percent; * = 10 percent.

(a) Total income consists of AFDC, food stamps, and earnings (for the sample member and her husband or partner), and some other sources.

random assignment, and the two groups' employment rates at the end of the follow-up period were comparable.

Overall, 43 percent of experimentals and 45 percent of controls had worked at some point after random assignment. The jobs were typically of brief duration: About half the jobs that began within a year after random assignment ended within three months after they started (not shown in the table). The majority of jobs ended when the young mothers quit them, reportedly for such reasons as problems with supervisors and other job-related issues, lack of child care or a preference for being with their children, insufficient pay or inconvenient hours, and pregnancy.

- **Control group members averaged higher cumulative earnings than experimental group members over the 18 months of follow-up.**

Controls earned, on average, $342 more than experimentals during the 18 months of follow-up. The negative program impact on earnings was much more substantial among those women who initially had more work experience – the group for whom the opportunity cost of program participation could have been expected to be especially high. For example, among those who had been employed during the year before random assignment, controls earned about $700 more than experimentals during the follow-up period. In contrast, the group difference among those who had never worked before random assignment was under $200 (not shown in the table).

- **Over 80 percent of the women in each of the research groups were on welfare throughout the 18 months of follow-up; there were no substantial program impacts on AFDC receipt.**

Nearly all of the women in both groups received AFDC at some point during the follow-up period. Although controls were somewhat less likely than experimentals to have received AFDC during months 4 to 6 following random assignment, overall the two groups received welfare for a comparable number of months (about 16 of the 18 months, on average). More than 80 percent of the women in both groups were still on welfare at the time of the 18-month follow-up interview.

At the 18-month point, New Chance had no impacts on income sources, i.e., the extent to which the young mothers' incomes derived from welfare payments, earnings, child support, or other sources (not shown in tables). The two groups also had comparable amounts of total measured income, averaging about $800 in the month prior to the 18-month interview. However, the program did affect a non-cash contribution to family resources: Experimentals were more likely than controls to be living in public housing or to be receiving rent assistance at follow-up: 35 percent versus 30 percent, respectively (not shown in tables).

Taken together, the 18-month findings indicate that there was an initial opportunity cost of participating in New Chance. However, the group difference in cumulative earnings was fairly small, as were the effects on welfare receipt. The trend over time indicates that the experimental group had caught up with the control group in terms of employment by the end of the 18 months of follow-up.

- **Experimental group members were more likely than control group members to have been involved in a "skill-building" activity (defined as employment or being in school or training), but the two groups were equally likely to be in such an activity at the end of the follow-up period.**

Experiences Working and Looking for Work

Qualitative data collected for the New Chance monograph (Quint and Musick, 1994) suggest that young women's experiences in the world of work reflect both the expectations (sometimes mature, sometimes unrealistic) that young women brought to the workplace and the realities they encountered there – realities that included poor supervision and outright discrimination.

A motivated but extremely obese young woman (she lost over 50 pounds while she was in New Chance but subsequently regained them), **Kelly** successfully completed an office skills training course. She then spent three months looking for work but was unable to get a private-sector job. She was eventually hired by the welfare agency, first for an unpaid "workfare" position and then for a nine-month subsidized job, at which she was working at the time she was interviewed. Kelly found that working did not benefit her financially: Her earnings brought her one dollar over the income limit to receive assistance with child care expenses, and she had to pay $266 a month for care for her four-year-old son. Her rent, although still subsidized, rose from $18 to $204 a month, and she no longer received food stamps. Nonetheless, asked if there were times she would rather be on welfare, she replied firmly, "No." Kelly (and many other respondents) spoke of the feelings of independence derived from working, and the loss of self-esteem experienced while on welfare. Although Kelly's supervisor at the welfare agency was pleased with her performance, the position was a temporary one, with no guarantee that she would be "rolled over" into a permanent position; and her obesity (which seems at least in part a response to the stresses she faces) puts her ability to find a private-sector job in serious question.

After **Nina** received a certificate as a medical secretary, she held two part-time jobs before finding one she really wanted, working full-time at two franchises of a nationwide chain of opticians. Nina and another Mexican-American young woman were employed at the stores as optometrists' technicians; so were two white young women. All the optometrists were white. Nina was excited when she started, but she soon found that a supervisor automatically credited one of the white technicians for things Nina had done, despite the fact that the white young woman consistently came in late and chatted for hours on the phone with friends when the bosses weren't around. Nina also discovered that the optometrists seldom engaged the Mexican-American young women in friendly conversation. At the same time, Nina was undergoing a good deal of non-work-related stress: Her husband, who was in the military, was about to return from service in the Gulf War. The last straw at work came when she found out that she and the other Mexican-American employee were earning less than the two white women, although all four had been employed for about the same length of time – and that even with the raise she was scheduled to get, the differential would remain. Nina quit without giving notice, an act she now regrets; she says she should have spoken with a supervisor about the situation. In retrospect, she feels she was reacting to a number of pressures that made her "explode"; as she recalls, "At the time, I had strong feelings about quitting. I seen things and it hurt me. . . ."

(continued)

Experiences Working and Looking for Work (continued)

Natalie was referred by the state employment service to a position as a maid in a motel. She liked cleaning and feels she did good work – it took her only 15 minutes to clean a room that would take others 20 or 25 minutes – but she quit after two months, saying, "I just could not get along with the guy that owned it." For one thing, Natalie felt that the owner should have explained to her just how he wanted things done (e.g., how many bars of soap should be left, how the bed should be made) instead of leaving the training to another maid. She also felt that the way the owner ran the place created needless problems for her. For example, because he did not provide enough clean sheets early in the day, the maids had to clean all the rooms and then go back to make the beds, an unnecessary and time-consuming procedure that she worried would interfere with her ability to pick up her three children from day care. An incident concerning her children caused her to leave the job. She had gotten word from the day care center that one of her children was ill, but the motel owner was reluctant to let her leave, despite the fact that the other maids said they would cover her chores. When she brought a doctor's excuse back the next day, the owner wadded it up and threw it back over the desk. Natalie quit on the spot; two of the other motel employees quit the same day. Part of the problem, too, was financial: Natalie calculated that, considering the high cost of getting to work, it was costing her more to work than just to stay at home. Nonetheless, she says she would take another job at the same $5 an hour wage if the supervisor were a nicer person.

After dropping out of a paralegal training program, **Gloria** took a part-time (five hours a day, six days a week) job as a telemarketer with a water softener company. The job's main asset was that she could walk to work. She soon discovered that while she disliked one of her four supervisors, she disliked telemarketing itself even more: "I didn't like calling people. No way." Gloria particularly detested calling people at 8 A.M. on weekend mornings, often waking them and being met with hostility. Although the job paid $4.50 an hour with no fringe benefits, Gloria received a bonus for every survey she was able to complete over the phone and a $20 bonus for every home demonstration she was able to set up. Flouting company policy, she figured out a way to enhance her income: She called members of her large extended family to survey them and to arrange home demonstrations. She reasoned that she was justified in enlisting her relatives' cooperation because they, too, would get something for participating (e.g., movie tickets, a free dinner); asked if her relatives were thereafter hassled by the company, she acknowledged laughingly that she had never asked. Although Gloria calculated that she was better off working than on welfare by about $200 a month, she quit the job after two and a half months. Some of the other telemarketers had been absent so often that she had to work 12-hour shifts. She was pregnant at the time and got sick, and her doctor advised her to take a week off from work: "And they got mad. So I just didn't go back." (It was unclear whether Gloria called in advance to tell the company she wouldn't be coming in or simply returned from her week off with the doctor's note in hand.) After her second child was born, Gloria went back to school, and had just completed a bookkeeping training course at the time of the interview.

The rate of involvement in a skill-building activity was high in both groups, but more experimentals (94 percent) than controls (82 percent) had been involved in at least one such activity during the 18 months of follow-up. The discrepancy peaked during the second month after random assignment and gradually diminished as experimentals left New Chance. By the time of the follow-up interview, just over one-third of the women in each group were engaged in a skill-building activity. Although this rate is lower than might have been hoped for, it should be noted that the majority of these women were still caring for a child under 2 years of age.

Impacts for Different Subgroups and Sites

Many previous evaluations have found that program impacts vary for different subgroups of people. For example, interventions that are effective for women with strong basic skills are not necessarily equally effective for women with weaker skills. Information on subgroup impacts may help planners target programs toward those who will especially benefit from them.

In the New Chance evaluation, experimental and control group differences were examined for subgroups of the sample, defined on the basis of a wide range of initial characteristics such as the sample members' age, ethnicity, number of children, reading levels, prior work experience, family welfare history, and time of sample entry. The subgroup analyses indicated that, by and large, program impacts were fairly consistent across subgroups; conversely, when impacts were not found for the full sample, they were generally not found for subgroups, either. Moreover, when subgroup effects *were* detected, they generally were not sustained across the various outcome areas. For example, the program's impact on the number of weeks spent in an education activity was especially large among Hispanic women: Among Hispanics, experimentals spent an average of 16 more weeks than controls in an education program (compared to an impact of 12 weeks for blacks and 8 weeks for whites). However, the program's impact on GED receipt was comparable for all three groups. Thus, the New Chance subgroup analyses at the 18-month point did not suggest that further targeting was in order.

Impacts were also examined at individual program sites. Certain sites had especially powerful impacts on GED attainment, but generally did not achieve impacts in other areas. In general, no site stood out as much better than the others across the full range of impacts.

Early Lessons from the New Chance Demonstration

Implications for Future Impacts. At 18 months after random assignment, the findings on the impacts of New Chance were mixed, but generally less positive than had been hoped for. However, it is difficult to predict what will happen in the longer run. The lives of these young, highly disadvantaged mothers, like the lives of most young people, are still in flux: Over the brief period of a year and a half, the majority had stopped and started one or more programs, and over half had become pregnant; substantial minorities of sample members had stopped and started one or more jobs, had had a baby, and had changed living arrangements at least once. Given this volatility, it is too soon to know whether the early positive impacts will eventually outweigh the negative ones, or vice versa. For example, if the higher pregnancy rate among experimentals eventually results in a birthrate higher than that for controls, will this adversely affect employment and earnings, or will effects on these outcomes be more than offset by the experimentals' higher rate of GED attainment? The 42-month findings will provide information regarding the longer-term effects of New Chance.

The 42-month findings will also provide the first evidence regarding impacts on the development of the children in the sample – children who at that point will be about 5 years old, on average. Here again, the 18-month impacts do not provide a clear-cut basis for predictions. Some of those findings augur well for later impacts on the children's development (e.g., the positive impact on emotional support in the home). However, several of the positive impacts were modest, and the impacts in other areas (e.g., child care) do not offer an unequivocal indication that enhanced development will ensue – especially since the children, too, are affected by the volatility of their mothers' lives.

While these families' lives were characterized by change and transience in many respects, it is noteworthy that their poverty, use of public assistance, and educational and social disadvantages persisted. The absolute level of disadvantage of these young mothers and their children needs to be taken into account in designing programs and policies to improve their self-sufficiency and life prospects.

Improving Program Design. Although the longer-term effects of New Chance are difficult to predict, the sites' experiences do suggest some directions for improving services to disadvantaged young mothers.

- **Attendance standards.** Given the low attendance rates of New Chance enrollees, it seems advisable for program staff to articulate clear attendance standards, and to enforce them through a system of rewards and penalties.

- **Family planning.** Programs for young mothers have been notably unsuccessful in affecting young mothers' fertility behavior. Programs may have more success if family planning receives ongoing, regular follow-up by program staff who feel comfortable discussing sexuality, and if an unequivocal message about postponing pregnancies is delivered continuously.

- **Employment components.** To encourage work, all the program activities need to emphasize how they relate to getting and keeping a job. It is likely that job development and job placement need to be aggresive and ongoing. The rapid turnover of the young mothers who worked also suggests the need to pay more attention to job-retention skills.

- **Post-placement and other follow-up services.** For many young women who moved into employment or another program after Phase I, the transition was difficult and dropping out was common. Regular and frequent counseling to resolve various problems would likely have proved beneficial.

- **Parenting.** Programs can make parenting education more appealing by emphasizing mother-child interaction rather than by direct instruction and by using peer facilitators.

- **Mental health and substance abuse services.** Several sites found it useful to augment service offerings beyond those prescribed in the program model by forging linkages with mental health providers, in order to deal more effectively with enrollees' mental health problems and with the substance abuse issues of the young women themselves or of their family members and partners.

- **Outreach to significant others.** Because family members and boyfriends play a crucial role in participants' lives and decisions, programs like New Chance need to find ways of enlisting their support for program goals.

It should be noted that the findings generally suggest that an integrated, comprehensive service model such as New Chance results in a higher level of service receipt. Despite the generally high level of services received by control group members, women in the experimental group received more services (and had higher service intensity) than did controls.

However, the sequential arrangement of services at most sites meant that many non-GED earners, and some GED earners as well, did not move on to skills training or a work internship. For these young women, consequently, New Chance was less employment-oriented than had been planned. Programs may want to explore ways of integrating skills training with education, to help ensure that the preparation for employment remains at the forefront of participants' awareness and their daily activities.

New Chance and Public Policy. Because New Chance served a group of young women at high risk of long-term welfare receipt, it is important to consider possible implications of the findings for welfare policy. Many of the welfare reform proposals currently under discussion would involve major changes to the entire welfare environment – changes that would substantially alter the incentives for working or remaining on aid. The early New Chance findings do not directly address these proposals, but they raise questions that should be considered in designing and implementing welfare initiatives as they pertain to young mothers, especially those who are high school dropouts:

- **Time limits on welfare receipt.** The New Chance findings indicate that interruptions to school, training, and work are commonplace in this population, and that most young mothers are still on welfare 18 months after enrollment. Given the young women's level of maturity, personal and academic problems, and responsibility for the care of young children, it is not clear that even a substantial amount of services, delivered in a single "dose," comports well with the realities of their lives, or that even a firm mandate would induce them to make the transition to stable employment in a two-year period, which has been proposed as a time limit for welfare receipt. The data suggest that a follow-on to time-limited welfare, such as a community work program, will be essential to avoid destitution on a large scale.

- **Exemptions and deferrals.** A major consideration with which welfare programs will need to contend is whether an exemption or deferral will be granted for pregnancy. Within a two-year period, it seems likely that many young mothers would become pregnant again and have another child.

- **Job turnover.** A substantial minority of young mothers were employed at some point during the 18 months of follow-up, but jobs tended to be of short duration. If time limits are imposed and quick job turnover remains the norm, policymakers will need to decide how to handle welfare assistance during periods between employment.

- **Skilled staff.** The difficult psychological and interpersonal problems faced by many young mothers – problems that frequently interfere with progress toward self-sufficiency – suggest that staff members of welfare-to-work programs will need counseling expertise and experience. Regular in-service training to help staff members deal with especially difficult issues is also indicated.

- **Child care.** The New Chance findings suggest that disadvantaged young mothers with small children *can* be encouraged to participate in skill-building activities, but it is clear that child care is needed for them to do so. Formal child care arrangements might enhance children's development, but if used extensively, such care would be very expensive.

- **"Making work pay."** Current policies regarding child care assistance may impede a transition to employment among young mothers with small children: Child care subsidies are generally not available for part-time employment and often requires mothers to prepay child care expenses and obtain reimbursement later. It also appears that many women who are working are mixing earnings and welfare. This suggests the usefulness of policies and incentives that allow recipients to increase their employment and earnings while working their way off welfare.

- **Living arrangements.** The New Chance findings do not address the issue of whether requiring young mothers who are minors to live with a parent would reduce the incentive to become pregnant. However, the findings do indicate that those living with a parent were not necessarily better off economically, educationally, or psychologically than those who chose an alternative arrangement.

- **Mandatory versus voluntary.** Few program enrollees felt a reciprocal obligation to participate in New Chance or another program as a condition of welfare receipt. Such a mandate might well increase participation. But it is not clear that a broad-coverage, mandatory program can induce participation for young mothers who have dropped out of school in a way that leads to increased self-sufficiency. Some may be positively affected by a participation requirement, while others (along with their children) may be harmed by repeated sanctions for noncompliance.

Along with these issues, the difficulty of changing behavior once young women have become mothers suggests the importance of giving increasing attention to efforts geared toward primary prevention of a first birth. And the fact that nearly 40 percent of the sample had dropped out of school before becoming pregnant indicates the need for education reforms to make schools more responsive to the needs of disadvantaged students.

Additional lessons for both policy and practice are likely to emerge as the New Chance story unfolds in the years ahead. The 42-month report will examine the experiences of young women who are no longer adolescents, and whose children are entering school.

CHAPTER 1

INTRODUCTION

I. The New Chance Demonstration: An Overview

This report presents short-term findings on the effects of a national demonstration program, New Chance. The demonstration tested the effects of an intervention aimed at addressing some of the most compelling social issues facing the country: the long-term poverty and welfare receipt frequently associated with early childbearing, as well as the reduced life prospects that young single mothers often pass on to their children. Specifically, New Chance sought to help its participants – young mothers aged 16 to 22 almost all of whom gave birth as teenagers, were recipients of Aid to Families with Dependent Children (AFDC, the main cash welfare program), were high school dropouts, and volunteered for the program – to increase their academic and vocational skills so that, over time, they could find and keep jobs offering opportunities for advancement and reduce their receipt of public assistance. It also sought to help participants acquire the motivation, knowledge, and skills to delay further childbearing, become better parents, and improve the quality of their decision-making and communications with family, friends, and the wider world. Finally, it aimed to enhance the cognitive, social, and physical development of participants' children. To these ends, the program model called for intensive, four- or five-day-a-week participation in a wide array of services.

New Chance operated at 16 locations in 10 states across the country. As shown in Table 1.1, the 16 local New Chance programs (also referred to in this report as the research "sites") were operated by a diverse group of program sponsors including community service organizations, schools and school districts, a community college, a Private Industry Council (the nonprofit entity that administers funding under the federal Job Training Partnership Act, JTPA), and an agency overseen by the county government; one program – Portland (Oregon) – represented a unique collaboration between a school district and the Job Corps.[1] Between late 1989 and mid-1992, these sites enrolled more than 1,500 young women – the *experimental* group – randomly selected from among program applicants. Their experiences are compared with those of a second group of women who also applied for the program but were instead randomly assigned to a *control* group, whose members did not have access to New Chance services.[2] Members of both groups were followed up through in-person

---

[1] All but three of these sites are operating a program like New Chance as of this writing. The Chula Vista (California), Bronx (New York), and Inglewood (California) New Chance programs closed because of funding difficulties. These closings did not materially affect the program experiences of the young women at these sites for whom data are presented in this report. However, this report has been written mostly in the past tense because it describes structures and activities as they existed or occurred during the time period covered by this study, essentially from late 1989 until mid-1993.

[2] As noted in Chapter 2, all program applicants filled out a New Chance Enrollment Form prior to being randomly assigned to the experimental group or the control group. However, only those assigned to the experimental group were actually allowed to enroll into the New Chance program. Thus, the terms *enrollees* and *experimentals* refer to the same group – those young women who were given access to New Chance services – and are used interchangeably in this report.

TABLE 1.1

THE NEW CHANCE PROGRAM OPERATORS

Location	Program Operator	Type of Organization	Prior Emphasis
California (Chula Vista)	Del Rey Center, Sweetwater Union High School District (a)	Adult school	Adult education
California (Inglewood)	Youth and Family Center (a)	Community service organization	Counseling, health services
California (San Jose)	Independence Adult Center, East Side Union High School District	Adult school	Education
Colorado (Denver)	Technical Education Center–North Campus, Community College of Denver	Community college	Adult education, occupational skills training
Florida (Jacksonville)	The Bridge Family Health Services, Inc.	Community service organization	Family planning, health services, tutoring
Illinois (Chicago Heights)	Aunt Martha's Youth Service Center, Inc. (b)	Community service organization	Counseling, education, employment preparation, health services
Kentucky (Lexington)	The Family Care Center (c)	Agency overseen by county government	Prevention and treatment of child abuse and neglect
Michigan (Detroit)	Development Centers, Inc., Community Mental Health Center	Community service organization	Mental health services
Minnesota (Minneapolis)	RESOURCE, Inc. (d)	Community service organization	Occupational skills training, employment preparation
New York (Bronx)	National Puerto Rican Forum, Inc. (a)	Community service organization	Adult education, English as a Second Language, occupational skills training
New York (Harlem)	Mid–Manhattan Adult Learning Center Office of Adult and Continuing Education New York City Board of Education	Adult school	Adult basic education, GED preparation, occupational skills training
Oregon (Portland)	PIVOT–New Chance Program Portland Public Schools	School–Job Corps collaboration	Education
Oregon (Salem)	Teen Parent Program The YWCA of Salem	Community service organization	Recreation, education, child care, counseling, health services
Pennsylvania (Allentown)	Expectant and Parenting Youth Program Private Industry Council of Lehigh Valley	Private Industry Council	Education, life skills, personal development
Pennsylvania (Philadelphia)	Lutheran Settlement House Women's Program	Community service organization	Adult education, life skills
Pennsylvania (Pittsburgh)	Pittsburgh in Partnership with Parents, Hill House Association	Community service organization	Comprehensive services for teenage parents

NOTES: (a) A New Chance program is no longer in operation at this site.

(b) This agency is now located in Park Forest, Illinois.

(c) The Family Care Center is a semiautonomous agency under the oversight of the Lexington–Fayette Urban County Government's Department of Social Services' Division of Family Services.

(d) This agency was formerly named Multi Resource Centers, Inc.

interviews conducted 18 months after entry into the research; this report discusses the results.[3]

New Chance was distinctive in bringing together three service traditions: (1) services designed to improve the "human capital" (i.e., education and job-related skills) of disadvantaged women and enable them to become economically self-sufficient; (2) programs designed to enhance the personal resources and parenting abilities of teenage mothers; and (3) programs aimed at improving the development and life trajectories of disadvantaged children. For these reasons, the New Chance findings are of interest to a broad audience of policymakers, program operators, researchers, and others concerned with early childbearing. Several specific characteristics of the program model heighten that interest:

- New Chance served a population that is at the core of national concern about welfare. While the young mothers in New Chance were notably diverse, their poverty, failure to have completed high school, and (in most cases) single-parent status all placed them at high risk of long-term welfare receipt, as discussed below.

- The treatment New Chance offered followed a comprehensive and intensive model of the kind many experts have recommended, with sites having delivered education, employment-related, health, parenting, and case management services mostly at the program site – a "one-stop shopping" approach designed to facilitate participation. Free child care is also part of the model, and nine sites operated regular on-site child care centers.[4]

- The New Chance model is explicitly "two-generational" in its method and philosophy, seeking to improve the well-being of participants' children as well as that of the mothers themselves.

- States can use the New Chance approach as one option for providing services to young welfare mothers through the federal-state Job Opportunities and Basic Skills Training (JOBS) Program, discussed later in this chapter.

- The evaluation of New Chance, which includes analyses of the program's implementation and operation, effects (or "impacts"), and benefits compared to costs, is yielding hard evidence on the value of this comprehensive approach in assisting an unusually at-risk group of young families.

The findings presented in this report, although based on relatively short-term (18-month) follow-up, are especially relevant in a policy context in which proposals to make receipt of welfare benefits without work time-limited and to require young mothers on welfare to live with their own mothers (or with other older adults) have received considerable attention.

---

[3] The evaluation's final report, scheduled for completion in 1996, will address the longer-term impacts of New Chance and its monetary benefits in relation to costs.

[4] Two additional sites provided child care on a temporary basis only, to give new entrants a place where their children could be cared for until they had made more permanent arrangements, or to serve as an emergency backup if their regular child care fell through. A twelfth site had an on-site child care center, but no slots were reserved for the children of New Chance enrollees, and very few enrollees used it. Consequently, it is not counted here.

The New Chance Demonstration has been funded by a broad consortium whose members are listed in Table 1.2. The program model and demonstration were developed by the Manpower Demonstration Research Corporation (MDRC), a private nonprofit organization that develops and tests initiatives to improve the well-being and self-sufficiency of poor people. The program model was based on consultations with some 30 experts in the field (including youth program operators, welfare administrators, and academicians) as well as a review of the program literature. The model was further refined after a six-site pilot test, which lasted more than a year and indicated the basic feasibility of the approach. MDRC designed and carried out the research agenda, provided ongoing technical assistance to the demonstration sites, and monitored their compliance with the program model and the research requirements.

This report, which presents data on the impacts of New Chance at 18 months after individuals entered the research sample, is the third on the demonstration. The first, a report on program implementation, described program start-up and contained early findings on enrollees' characteristics and participation.[5] The second was a monograph based on in-depth interviews with 50 young women two and a half years (on average) after they had left New Chance; it explored their life circumstances and activities during the period after leaving the program.[6]

The earlier reports point to several key themes that are important to keep in mind in reading this report as well. First, at the outset of the demonstration, sites confronted many simultaneous challenges: developing the program infrastructure and components, recruiting enrollees, and establishing program policies and rules. Second, there was significant variation among the young mothers in terms of personal strengths and social supports: Some came from solid, working-class families, while others came from long-term welfare families and had experienced extreme poverty. Third, many faced an array of personal and situational problems – such as conflicts with parents and partners, unstable living arrangements, domestic violence, and substance abuse – that interfered with their ability to participate in New Chance and to realize the program's goals. Fourth, the young women's lives were frequently changing as they moved in and out of programs, jobs, and relationships, and their progress was nonlinear, with spells of active engagement in school, training, or work interrupted by periods during which they were involved in none of these activities.

Finally, many of the young women were still adolescents – emotionally, if not chronologically. Research conducted for the monograph suggests that many of the young mothers were themselves poorly nurtured while they were growing up, and their need for affection and stability may have left them vulnerable to early sexual activity and parenthood, while failing to prepare them for adult life and its responsibilities. Indeed, premature parenthood may itself have disrupted the young women's ability to focus on their own development and maturation. A challenge facing New Chance, along

---

[5] Janet Quint, Barbara Fink, and Sharon Rowser, *New Chance: Implementing a Comprehensive Program for Disadvantaged Young Mothers and Their Children* (New York: MDRC, 1991).

[6] Janet Quint and Judith Musick, *Lives of Promise, Lives of Pain: Young Mothers After New Chance* (New York: MDRC, 1994).

TABLE 1.2

FUNDERS OF THE NEW CHANCE DEMONSTRATION

U.S. Department of Labor
Ford Foundation
W. K. Kellogg Foundation
DeWitt Wallace-Reader's Digest Fund
Meyer Memorial Trust
The UPS Foundation
Charles Stewart Mott Foundation
The Pew Charitable Trusts
Stuart Foundations
William T. Grant Foundation
The Skillman Foundation
The David and Lucile Packard Foundation
AT&T Foundation
The Bush Foundation
Foundation for Child Development
Exxon Corporation
The Chase Manhattan Bank, N.A.
Koret Foundation
ARCO Foundation
GE Foundation
National Commission for Employment Policy
Mary Reynolds Babcock Foundation
The Allstate Foundation
Neighborhood Reinvestment Corporation
Honeywell Foundation
Grand Metropolitan Foundation
Kaiser Permanente
Anonymous Funder

---

with other programs for disadvantaged young mothers, is to help them develop a strong, positive self-concept and, along with it, the self-discipline and skills to make good decisions and act on them.

The early impact findings contained in this report are mixed, and overall less than had been hoped for. New Chance had a positive and statistically significant impact on receipt of a General Educational Development (GED) certificate[7] and of college credits, and it also increased participation in many kinds of services. New Chance also had a positive, although small, effect on enrollees' parenting attitudes. However, young women in New Chance had significantly more pregnancies (although not more births) than their counterparts who were not in the program, and they were less likely to be using contraception regularly. (Their higher rate of pregnancy may be related to the fact that, at follow-up, they were more likely to be living with a male partner.) The two groups were equivalent with regard to many measures for which a positive impact had been desired, including reading skills, subsequent births, depression, drug use, and health. At this early stage, there were no impacts on either employment or welfare receipt. This report aims to explain, as well as to describe, these results insofar as possible.

These relatively short-term impacts, however, are just that – measures of the program's effects at a quite early point in time. They are far from the final story about the effectiveness of the intervention. The fact that the program population is still in the middle of the transition between adolescence and adulthood suggests that it may take some time – and greater maturity – for the young women to put into effect what they may have gained from participation in New Chance. This possibility will be examined in depth in the evaluation's final report, based on 42 months of follow-up; it is scheduled for completion in 1996.

This chapter sets the stage for the remainder of the report. The next two sections examine the issues associated with adolescent childbearing and the way in which welfare policy seeks to address these issues. In the fourth section of the chapter, the New Chance program model is described in greater detail; in the fifth, the research design is explained. Then, the experiences of other programs for young mothers are reviewed, to establish a context in which the findings of this report can be understood. The chapter concludes with an overview of the rest of the report.

II. Adolescent Childbearing: The Issues

The past two decades have witnessed marked changes in the prevalence and patterns of teenage childbearing in the United States. The birthrate among teenagers dropped from 68 births per thousand for women aged 15 to 19 in 1970 to 50 in 1986 (Moore, 1993), a decline generally thought to be attributable to the legalization of abortion (U.S. General Accounting Office, 1986). Since then, however, teenage birthrates have risen yearly: In 1991 (the most recent year for which national data are available), there were 62 births per 1,000 women aged 15 to 19 (National Center for Health Statistics, 1993). Moreover, the percentage of births to unmarried teens has climbed dramatically. In 1970, only 30 percent of births to teenagers were to unmarried women; in 1990, 68 percent of the half million births to mothers aged 19 and under occurred outside of marriage. Finally, with contraceptives more widely available and abortion now legal, it seems likely that teenagers who give birth today are different from their earlier counterparts: They may want children more, or be more disadvantaged, economically and otherwise, so that they may perceive themselves as having very limited futures in any case and thus may see fewer drawbacks to early childbearing.[8]

---

[7] This credential is given to those who pass the GED test and is intended to signify knowledge of basic high school subjects.

There is considerable evidence that teenage childbearing is associated with a host of negative life outcomes. Although there is controversy over the extent to which these outcomes are *caused* by an early birth (Furstenberg, 1991; Geronimus and Korenman, 1992, 1993; Hoffman, Foster, and Furstenberg, 1993), there is little dispute that young mothers are disadvantaged educationally, economically, and socially.[9]

Pregnancy is a major reason girls give for dropping out of high school (Ekstrom et al., 1986). Nearly half of the women who first gave birth at age 17 or younger during the early 1980s failed to complete high school, compared to under 10 percent of those who postponed childbearing until their early twenties (Upchurch and McCarthy, 1990); young mothers who dropped out before becoming pregnant were especially unlikely to complete their educations (Upchurch, 1988). Ahn (1994), who used a complex hazard model to control for individual and family effects, concluded that a teenage birth itself is associated with a reduction of about 50 percent in the likelihood of high school completion.

Given their lower educational attainment, as well as deficiencies in basic skills (Berlin and Sum, 1988), young mothers find themselves at a distinct disadvantage in the labor market. There is extensive evidence that young mothers have lower labor force participation (especially early on), lower earnings, less prestigious jobs with fewer opportunities for career advancement, lower family incomes, and higher rates of poverty than women who give birth at a later age (Hofferth, Moore, and Caldwell, 1978; Haggstrom et al., 1981; Duncan and Hoffman, 1990; Grogger and Bronars, 1993; Hoffman, Foster, and Furstenberg, 1993).

While their educational deficiencies contribute heavily to teenage mothers' economic situation, another important consideration is that they have more children than women who postpone their first birth (Hoffman, Foster, and Furstenberg, 1993; Hofferth, 1987; Heckman, Hotz, and Walker, 1985). This difference in family size is also associated with both reduced labor force participation and increased poverty (Furstenberg, Brooks-Gunn, and Morgan, 1987). Extended welfare receipt is another concomitant of adolescent pregnancy, as discussed in the next section.

---

[8] It should also be noted that certain changes in the social context of teenage childbearing over recent decades might be expected to result in improved outcomes for young mothers: the passage of legislation to ensure teen parents the right to remain in school, and the expansion of services to pregnant and parenting teenagers to enable them to do so. See Nord et al., 1992.

[9] The work of Geronimus and Korenman has made it clear that the studies that appeared in the 1970s and early 1980s exaggerated the independent effect of an early birth on the social and economic outcomes for young women. However, many researchers are now using considerably more sophisticated statistical methodologies and research designs and are finding that the negative effects associated with early childbearing cannot be attributed solely to selection, i.e., that the women who gave birth as teenagers would have had the negative outcomes in any event because of high initial levels of disadvantage. See, e.g., Ahn, 1994; Grogger and Bronars, 1993; Hoffman, Foster, and Furstenberg, 1993. The preponderance of evidence appears to indicate that early childbearing is often detrimental to the life chances of already disadvantaged women.

Children of adolescent mothers have been found to be at higher risk of developmental problems than children of older parents. Children born to young mothers are not only more likely to live in poor families headed by a single parent, but they are also more likely to be reared by mothers who are less competent as parents. Researchers have found, for example, that teenage mothers are more likely than older mothers to have unrealistic expectations regarding children's developmental milestones, less likely to attend to their children's needs for verbal and other forms of cognitive stimulation, and more likely to be hostile and punitive toward their children (Roosa, 1983; Garcia-Coll, Hoffman, and Oh, 1987; Landy et al., 1983; Levine, Garcia-Coll, and Oh, 1985; Parks and Arndt, 1990). Given these circumstances, it is not surprising that researchers have also found that children of young mothers are raised in considerably less favorable home environments (Luster and Rhoades, 1989; Hannan and Luster, 1991; Polit, 1992a). Thus, teenage parenthood is a social problem with intergenerational dimensions.[10]

Finally, while recent studies confirm that early parenthood has adverse effects on outcomes, these effects have often been shown to be indirect. For example, age at first birth does not affect labor market outcomes directly; rather, early childbearing reduces educational attainment and increases family size, and it is these outcomes that negatively affect workforce participation and wages. Thus, the research suggests specific avenues for program intervention: If programs can boost educational attainment and postpone subsequent pregnancies among participants, then negative labor market outcomes may be forestalled.

III. Young Mothers and Welfare: The Problem and the Policy Response

Welfare receipt is also strongly associated with early childbearing (since able-bodied young women over age 18 who do not have children would find welfare almost impossible to obtain in many localities), and it is a consequence that is costly to both the young mothers and the public. One recent study (Adams, 1990) estimates that three-quarters of all unmarried teenage mothers receive AFDC within five years of their first child's birth. Welfare, to be sure, is an important income support for young mothers, but one that comes at a price for its recipients: a standard of living at or below the poverty line and social stigmatization.

Young mothers' receipt of welfare is costly to society as well. Teenage mothers represent a relatively small proportion of all mothers on welfare – 8.1 percent of the average monthly caseload during fiscal year 1991 (U.S. Congress, House Committee on Ways and Means, 1993). But because early childbearing increases the likelihood that a young woman will receive AFDC for an extended period, the majority of welfare expenditures go to households begun by a teenage mother; it is estimated that in 1991 the combined cost of public assistance, food stamps, and Medicaid for such households exceeded $29 billion (Center for Population Options, 1993).

---

[10] New Chance is one of the earliest large-scale demonstrations to have addressed the needs of both disadvantaged mothers and their children, but an increasing number of programs are being designed with such a two-generational focus. Some link the JOBS program for welfare recipients to high-quality child care or to the Head Start program. Others were developed apart from JOBS and are directed to low-income families that are not necessarily on welfare (Smith, 1991).

Lengthy welfare stays are especially likely for women who are high school dropouts. Bane and Ellwood's (1983) pioneering analysis of data from the Panel Study of Income Dynamics (PSID) indicated that non-white women who went on welfare after giving birth as unmarried mothers and who were high school dropouts averaged 10 years on the welfare rolls. In a further analysis of these data that took into account multiple spells on welfare, Ellwood (1986) concluded that to reduce long-term receipt of public assistance, programs should target never-married women aged 25 and under who go on welfare when their child is less than 3 years old.

New evidence suggests that multiple welfare spells and cycling on and off the welfare rolls are common among young mothers. Using data from the National Longitudinal Study of Youth (NLSY), which contains monthly information on welfare status,[11] Pavetti (1992) followed 424 young women who first received AFDC benefits when they were aged 20 to 23 for five years after the start of their first welfare spell. She found that over this five-year period, 58 percent of all welfare spells experienced by high school graduates, and 40 percent of the welfare spells experienced by high school dropouts, ended with a work exit, but that 60 percent of the women who left welfare for work returned to public assistance, often within the first year after leaving it.

The policy context in which New Chance was originally developed and in which it has unfolded has been marked by continuing Congressional and public concern about the fiscal, social, and personal costs of long-term welfare receipt. The Family Support Act of 1988 gave legislative voice to that concern, and six years later, welfare reform remains at the forefront of the domestic social policy agenda.

The Family Support Act signified a broad commitment to the concept that receiving welfare entails mutual obligations: a responsibility on the part of the recipient to participate in services that will help her to support herself and her children, and a duty on the part of government to provide these services.[12] A major provision of the Family Support Act was the creation of the Job Opportunities and Basic Skills Training (JOBS) Program, which gives state welfare agencies increased funding and incentives for delivering education, vocational skills training, job-readiness activities, job placement, and other employment-related services, either directly or through contracts with education, job training, and other agencies. Title III of the Family Support Act requires that child care and transportation be available if needed to AFDC recipients mandated to participate in JOBS and provides for child care for all AFDC recipients (regardless of JOBS participation) who need it to become or remain employed. The legislation also extends eligibility for Medicaid and for subsidized child care for one year after a family is terminated from AFDC because of increased earnings.

To the extent that the requisite employment-related services and child care are available, states must require all AFDC household heads whose youngest child is 3 or older (age 1 or older at state option) to participate in JOBS unless they are deemed exempt (e.g., because they have other responsibilities that require them to stay at home, such as caring full-time for a disabled child, or because they are already working 30 hours a week). Failure to participate without good cause may result in a reduction of the welfare grant (a "sanction," in welfare administrative parlance). The JOBS financial incentive structure is designed to encourage states to target groups considered most likely to become long-term welfare recipients, including families headed by a parent under the age of 24 who has either no high school diploma or little or no work experience. Within each target group, states are directed to serve volunteers first; and, indeed, because of resource constraints, some states have operated essentially voluntary JOBS programs.

---

[11] The PSID, in contrast, notes only whether or not an individual received welfare at some point during the year.

[12] Enhanced child support enforcement provisions contained in the Family Support Act gave legislative expression to the expectation that fathers as well as mothers should support their children.

In order to prevent high school dropout or to induce young mothers who have already dropped out to complete their schooling, states must require that teen parents without a high school diploma or GED attend school or other education programs, regardless of the age of their children, to the extent that resources permit. Because of financial constraints, however, it appears that few states have implemented such requirements on a large scale.[13]

The New Chance evaluation contributes to a growing but still small body of evidence about the effectiveness of program approaches for young mothers under JOBS. As noted earlier, New Chance is a program option that states can implement using JOBS funding, and the majority of its enrollees would be considered JOBS-mandatory if the state or local JOBS program opted to require teenage mothers to participate in education services.[14] Most New Chance sites have used JOBS funding to pay for occupational skills training, support services (e.g., child care or transportation), or program staff. Because JOBS was phased in over time, at the outset of the demonstration, New Chance enrollees were not subject to a JOBS participation requirement. (At some sites, this changed over time, and, as discussed in Chapter 8, a small percentage of program enrollees had their grants reduced because of poor attendance.)

The findings of this study, as also noted earlier, have implications for the current debate concerning welfare reform. Recent reform proposals have centered on the imposition of a time limit (two years is most commonly advanced) on the receipt of benefits without work. During that time, adult welfare recipients would be both encouraged and required to participate in education, job training, and other services aimed at increasing their employment. In some plans, if they had not found a regular job by the end of the period, they would be required to take a community service job in order to continue to receive aid. Other proposals have been aimed specifically at young mothers

---

[13] Twenty-five states responded to a 1992 survey conducted by the Center for Law and Social Policy to determine the proportion of AFDC teen parents in the state who were enrolled in JOBS. Of the 25 reporting states, four (Florida, North Carolina, Ohio, and Oklahoma) accounted for over half of all teen parents in JOBS. Eight states reported that over 35 percent of the teen parents on their AFDC caseloads were JOBS enrollees. However, some states considered teen parents "in JOBS" if they were on the "active caseload," whether or not they were actively engaged in a component such as education or job training (Levin-Epstein, 1993).

[14] At the time they entered New Chance, 69.6 percent of New Chance enrollees could have been mandated to participate in JOBS if the states they lived in so required: 64.3 percent were under age 20 and did not have a high school diploma or GED, while 5.3 percent were 20 or older and had no children under age 3. However, an enrollee's status vis-à-vis JOBS could change during the course of her participation.

– e.g., a requirement that they live with their parents or with other adults as a condition of receiving welfare.

This study cannot speak to what young mothers *would do* in the presence of time limits and mandates, but it contains a great deal of information about what New Chance sample members, at least, *have done* in their absence. It highlights some of the complexities that need to be addressed in translating broad policy initiatives into specific programs and regulations. A few examples may clarify this point, which is discussed in more detail in Chapter 9. In New Chance, participation in education and training was often interrupted by other events in the young women's lives, including pregnancy. Such information is relevant to the question of how a time limit would be enforced, and under what circumstances deferrals would be granted. New Chance enrollees experienced a substantial amount of job turnover, suggesting that this is an issue with which community service employment programs might have to deal, and that appropriate ways to reduce potential turnover might be sought. New Chance enrollees in various living arrangements had very different outcomes, and no one living arrangement was unequivocally superior to the others. This suggests that accurately predicting the behavioral effects of a particular residency requirement may be difficult.

In short, while the study cannot provide definitive answers, it can focus policymakers' attention on important questions.

IV. The New Chance Program Model

The 16 New Chance sites generally adhered to a specific model of service delivery. MDRC provided the sites with detailed guidelines concerning criteria for program eligibility and attributes of the treatment itself (i.e., the service offerings and structure and the program operating environment). Within these parameters, the sites had some flexibility to organize activities in ways that met their own circumstances and the needs of their participants. Table 1.3 summarizes key features of the program model.

A. The Target Population

New Chance was directed toward young women aged 16 to 22 who gave birth as teenagers and who at enrollment lacked a high school diploma or GED and were receiving AFDC. It is estimated that monthly, between May and August 1993, there were some 271,000 young mothers on the welfare rolls nationwide who met these criteria and who were not enrolled in school (special calculations from the U.S. Bureau of the Census, Survey of Income and Program Participation).[15]

By targeting this group, program planners sought to address the needs of young women who, collectively, are at unusually high risk of long-term poverty and welfare receipt and who, because of their relatively poor employment prospects, in the past had been typically overlooked by many welfare

---

[15] An additional 80,000 women were enrolled in school either full-time or part-time. However, because the source of these data – the Survey of Income and Program Participation – did not ascertain whether or not respondents had a GED, the 271,000 figure probably overestimates somewhat the number of young women who would have been eligible for New Chance.

TABLE 1.3

THE NEW CHANCE MODEL

Target Group

Mothers 16 to 22 years old who: (1) first gave birth at age 19 or younger; (2) receive AFDC; (3) do not have a high school diploma or GED; and (4) are not pregnant when they enter the program.

Treatment

Service Components:

Orientation

Phase I components
- **Education:** adult basic education, GED preparation
- **Employability development:** career exploration and pre-employment skills training
- **Health and personal development components:** Life Skills and Opportunities curriculum, health education and health care services, family planning, adult survival skills training
- **Components to enhance child development:** parenting education and pediatric health services

Phase II components
- **Employment preparation components:** occupational skills training, work internships, job placement assistance

Case management

Child care

Service Emphasis: integration and reinforcement in each component of all program messages and skills

Service Structure: sequential phases of program activities, relatively long duration (up to 18 months), high intensity, primarily on-site service delivery

Environment: small, personal programs; warm and supportive, but demanding, atmosphere

---

employment and job training programs. Recognizing that some young women might be very needy but might not meet all these criteria, MDRC allowed sites to enroll up to 25 percent of applicants who were high school graduates but read below the ninth-grade level, or who were economically disadvantaged but not on AFDC. Only 11 percent of program enrollees were admitted under this "window," however.

To be eligible, young women also had to be able to take full advantage of the program's services and then to make the transition to employment. For this reason, they could not be pregnant at the time of enrollment. Pregnant applicants were instructed to reapply after giving birth. However, applicants were not required to submit proof that they were not pregnant, and a few pregnant women were admitted to the program; they may not yet have been aware of the fact, or they may have chosen to conceal it from program operators.

B. The Program Treatment

The New Chance model adopts a holistic approach aimed not only at building participants' human capital but also at helping them become mature, confident, and healthy adults and parents. Along with providing specific services, program staff are expected to strive to build enrollees' self-esteem and their belief in their own ability to change their lives for the better.

As Table 1.3 shows, the model calls for the program treatment to begin with an orientation, sometimes lasting several days. During a participant's first several months in the program – "Phase I" – most services are delivered at the program site. Depending on her level of academic skills at entry, she is assigned to either adult basic education (i.e., instruction in reading, math, and composition) or classes preparing her to take the GED test. Typically, the education activity occupies two to three hours of a participant's six-hour day during this phase. Employability development classes are devoted to such topics as possible careers and job-seeking techniques. A variety of personal development services are also offered during Phase I: health education classes and, at some sites, health care services; family planning instruction; Life Skills and Opportunities (LSO) classes (classes using a curriculum developed especially for the New Chance Demonstration and emphasizing decision-making and communication skills); and "adult survival skills" (topics such as budgeting or legal rights and responsibilities that are sometimes included in other components and sometimes covered in a separate class). Finally, in keeping with the program's two-generational focus, Phase I includes services to promote child development: parenting classes and pediatric health care services.

During Phase I, the New Chance schedule is much like that of a regular school: Typically, the program runs from 9 A.M. to 3 P.M. for five days a week.[16] Daily attendance at all classes is expected. In other respects, however, New Chance is designed to be very different from high school. For one thing, the services mandated in the New Chance program design are distinct, but they are also intended to be integrated. Components are expected to complement and reinforce one another and to present participants with a consistent set of program messages interwoven through all program activities. Thus, for example, in a parenting class, students might make notes about their children's attainment of developmental milestones, thereby gaining additional practice in writing; or in a life skills class, students might develop child care budgets for two versus three children in order to get a clearer picture of the financial costs of having another child.

---

[16] Five sites opted for a four-day-a-week schedule, reasoning that if participants had a specific time for appointments (e.g., with doctors or welfare workers), they would be less likely to miss other classes. Staff at these sites typically used Fridays for meetings and preparation of lesson plans.

Even more important, local programs are intended to be small so as to promote an intimate and personal environment in which participants and staff can establish close attachments. Guidelines called for sites to enroll 100 participants over 12 to 18 months and to serve about 40 participants at any given time; case managers' caseloads were to include no more than 25 active participants (although they often exceeded this level in practice). Staff are expected to promote participants' development by creating an atmosphere that is supportive, with praise for both large and small accomplishments, but also demanding and marked by high expectations.

Receipt of a GED is envisioned as only the first step toward self-sufficiency. After a participant has received this credential (or if she has not earned a GED but has been in the program for five months),[17] the guidelines call for her to enter "Phase II," which could include vocational training, paid or unpaid short-term work experience (called "work internships"), and job placement.[18] In the demonstration, most of these activities took place away from the program site, although some sites offered on-site skills training. Case managers are expected to monitor participants' progress and to provide guidance and support not only while the young women are on site but also after they have moved on to training, college, or jobs. Young women can remain in New Chance for 18 months, with up to one year of additional follow-up by case managers. Throughout the 18 months, they are entitled to free child care.

It is important to note that, over time, the program model has continued to evolve in response to staff perceptions of participants' needs; this topic is discussed in later chapters.

V. The Research Agenda and Associated Issues

The evaluation of New Chance includes three major components. The *impact analysis* rests on a research design whereby young women eligible for the program were randomly assigned to one of two groups. Members of the *experimental* group were allowed to enroll in New Chance. Members of the *control* group were excluded from New Chance but were given a list of other programs and services available in their communities in which they were free to participate. Thus, the experiences of the controls were intended to reflect what is likely to have happened without New Chance.[19] The research plan called for survey interviews to collect comparable information from members of both groups at 18 and 42 months after random assignment. If the plan has been well implemented, then, by definition, the differences between the outcomes for experimentals and the outcomes for controls will be the *impacts*, or *effects*, of New Chance. The 18-month interview was designed to capture the relatively short-term impacts of program participation (reported in this document). The 42-month interview permits examination of the program's longer-term effects on the mothers; it also includes an assessment of sample members' children, to determine whether the program affects their cognitive and emotional development.

---

[17] The stipulation that participants move on to skills training or a work internship after five months was intended to keep young women focused on employment as the ultimate goal, as well as to prevent boredom and discouragement on the part of young women who were unsuccessful in earning a GED. However, sites did not systematically adhere to this guideline, as discussed in Chapter 3.

[18] College, although not formally considered a Phase II component, was also a post-GED activity for a number of young women.

[19] Allowing programs to provide controls with a list of service alternatives was deemed essential to win sites' compliance with the random assignment process. However, because these lists familiarized the controls with other service options and (by listing phone numbers or addresses) facilitated their access to these services,

(continued...)

The *process analysis*, also known as the *implementation analysis*, describes the New Chance population, the program treatment, and the way it was put in place at the program sites. It also analyzes patterns of program participation and retention for members of the experimental group. Finally, the *benefit-cost analysis* compares the costs associated with New Chance and the benefits that accrue from program participation with the costs and benefits incurred by members of the control group. These benefits and costs are measured from the perspectives of the participant, the taxpayer, and society as a whole. It should be emphasized, however, that not all the potential benefits are readily quantifiable: It is difficult, for example, to attach a dollar value to improved child development outcomes.

Because New Chance operated primarily as a voluntary program, the findings presented in this report cannot be used to infer what the results would be if the program were mandatory.[20] Also a matter for conjecture is how New Chance enrollees might have differed from other eligible young women who chose not to enter the program, because data on the circumstances and motivations of the latter group are not available. Young women who joined New Chance may have been more motivated and may have faced fewer obstacles to participation, at least at the outset. But it is also possible that some young women chose not to enroll because they felt they were doing well without the program's assistance, or because there were features of the program that did not appeal to them.

The fact that New Chance served volunteers has a second important consequence for the research: It means that if, at the outset, members of the experimental group were able and motivated to receive the kinds of services New Chance offered, so were their control group counterparts. The extent of service receipt by members of the control group is an important factor in explaining impacts (or their absence) in any evaluation. Service receipt by controls is especially critical in evaluations of voluntary programs in locations where alternative services are widely available because, it is assumed, those who voluntarily apply for a program are more likely to want the services offered and to seek them elsewhere if they are assigned to the control group. It is important to remember that the

---

[19](...continued)
controls may have received these services to a greater extent than they would have if lists had not been provided.

[20] Since New Chance targeted high school dropouts, and some of the young women it reached had more than one child, its enrollees may have faced greater barriers to self-sufficiency than did participants in mandatory demonstrations serving only first-time mothers or programs enrolling a wider range of young welfare mothers (including those who remained in school after giving birth and those who had already completed high school).

impacts presented in this report do *not* reflect what would have happened if controls had not received any services at all. Thus, the report does not test the value of the services per se. Instead, the research tests the value of a "packaging" strategy that was expected to result in a service *increment* for members of the experimental group above and beyond what controls would receive.

VI. Lessons from Other Programs for Young Mothers

To provide a backdrop for the findings presented in this report, it is useful to consider the experiences of other research and demonstration programs serving teenage mothers on welfare, focusing where possible on their records in reaching young mothers who were high school dropouts. Four such programs are particularly relevant:

- **The Learning, Earning, and Parenting (LEAP) Program.** Developed by the Ohio Department of Human Services and operated since 1989 by county Departments of Human Services, LEAP is an unusual statewide initiative that uses financial incentives and penalties to promote school attendance among pregnant and parenting teenagers on welfare. The program requires teenage mothers and pregnant teens who do not have a high school diploma or GED and who are on welfare to stay in school or, if they have dropped out, to return to school or enter a program to prepare for the GED test. It offers both positive and negative financial incentives for them to do so: A bonus is added to the household's monthly welfare grant to reward good attendance, while the grant is reduced to penalize poor attendance. By improving the teens' school attendance in the short term, LEAP seeks to increase the likelihood that they will complete school and, in the longer term, find jobs and leave welfare. (See Bloom, Fellerath et al., 1993.)

- **The Teenage Parent Demonstration.** Funded by the U.S. Department of Health and Human Services, this demonstration, like the LEAP program, offers an opportunity to study the effectiveness of mandatory-participation programs for teenage parents. Operated from late 1987 to mid-1991 in Newark and Camden, New Jersey, and in the southern part of Chicago, the demonstration was aimed at all teenage mothers with one child who were first-time recipients of AFDC. The young mothers were divided almost equally into those who were in school at program enrollment, those who were dropouts, and those who had already graduated. The teens were required to participate in job search, training, or education programs; failure to register for the program or to comply with this requirement could result in a sanction removing the teen's portion of the AFDC grant. In addition, teens received case management, child care and transportation assistance, and workshops on parenting and other topics. (See Maynard, Nicholson, and Rangarajan, 1993.)

- **The JOBSTART Demonstration.** This demonstration, implemented at 13 diverse sites across the country between 1985 and 1988, sought to increase employment and earnings among economically and educationally disadvantaged youth. Enrollees, who volunteered for the program, were between 17 and 21 years old and were high school dropouts reading below the eighth-grade level. About a quarter of them were young women with children, and about 60 percent of these young mothers received AFDC on their own case. The program provided participants with education services (instruction in basic academic skills and GED preparation) and vocational training, either concurrently or sequentially. All sites provided transportation and child care assistance; the availability of other services (such as life skills instruction) varied greatly from one site to another. (See Auspos et al., 1989; Cave et al., 1993.)

- **Project Redirection**. Project Redirection was directed toward teenagers aged 17 years or younger who lacked a high school diploma or GED and were either receiving or were eligible to receive AFDC. Some 60 percent of the enrollees were out of school at program entry. During the main demonstration phase, between mid-1980 and the end of 1982, the program operated at four sites, where an evaluation of the program's impacts was conducted; it was subsequently expanded to an additional seven sites. The program's objectives were to help participants (who volunteered for the program) to return to or remain in school, delay subsequent pregnancies, and acquire employability and life management skills. The program's strategy was to link participants with existing services in the community and to support these "brokered" services by providing workshops, peer group sessions, and individual counseling in the program setting. It also paired teens with adult mentors, "community women" who volunteered to provide ongoing support, guidance, and friendship to the teens. (See Levy, 1983; Polit, Quint, and Riccio, 1988.)

Later chapters of this report cite specific impacts of these programs on the outcomes of interest. At the outset, however, it may be useful to summarize six general lessons that the evaluations of these programs suggest:

1. Young mothers participating in these programs have confronted many serious obstacles to advancement. All four studies indicate that young welfare mothers face many barriers to success, quite aside from poor school records and early parenthood, and in all of the programs, many young women lived in neighborhoods that were marked by high unemployment and crime. Significant issues among enrollees in the Teenage Parent Demonstration, e.g., included high levels of depression, low self-esteem, distrust of others, dysfunctional family situations, a lack of stably employed or happily married role models, negative peer groups, and unsupportive attitudes of male partners. Among the 18 teens examined in depth in an ethnographic study of Project Redirection (Levy, 1983), at least two had been raised by abusive parents (and one was herself suspected of child abuse), a few were estranged from their mothers, two were frequently abused by their boyfriends, and one may have been involved in prostitution. Alcoholism and drug abuse were common in the homes of a significant minority of the teens and their parents. Strikingly, a large fraction of teens in LEAP saw their high schools as unruly and dangerous (Bloom, Fellerath et al., 1993).

2. Securing high levels of attendance has been an issue in mandatory and voluntary programs alike. In a typical month, about one-quarter of all LEAP teen mothers (including those enrolled in school at the outset as well as dropouts) had good enough attendance to warrant a bonus, while about one-fifth were slated for a sanction because they were not enrolled in school or their attendance was poor. In the Teenage Parent Demonstration, sanctions were also frequently levied because of noncompliance both with the initial registration requirements and with ongoing participation requirements.

The fact that a program serves volunteers does not mean that they can be counted on to attend regularly. JOBSTART program operators reported that a substantial proportion of the participants were frequently absent from classes, with some students routinely missing classes once or twice a week and others attending regularly for some weeks but then not showing up for a week or more at a time. In addition, 13 percent of the participant sample interrupted their participation but then returned to the program; for women, the average length of inactivity was about two months. School absenteeism was a significant problem among Project Redirection participants, with teens who were enrolled in GED programs attending only 50 percent of the time and those enrolled in regular or alternative high schools attending only about three-quarters of the time.

3. Program structure makes a difference in the amounts of particular services that enrollees receive. Some JOBSTART sites offered education and vocational training concurrently. At other sites, youths participated first in education activities and then, having received a GED, entered skills training, sometimes at the same agency and sometimes at a different one. Program structure was a major determinant of service receipt: Youths at "concurrent" sites were much more likely than those at "sequential" sites to receive skills training, especially when that training was delivered by a different agency than the one providing education. At the sequential sites, many young people did not participate in skills training because they dropped out of the program before completing the first phase; others completed the first phase but opted not to continue.[21]

4. Programs have had varying records in helping young mothers to secure education credentials and employment, but in most cases the majority of enrollees remained on welfare and in poverty. In the Teenage Parent Demonstration, there were large impacts on school enrollment at two years after sample intake; impacts on receipt of education credentials and on employment were much smaller and not always statistically significant. The vast majority of both experimentals and controls were living in poverty at the time of the follow-up surveys. Young mothers in JOBSTART were considerably more likely than young mothers in the control group to have earned a GED (or a high school diploma), but impacts on employment rates and earnings were seldom large enough to be statistically significant. Nor was there an impact on AFDC receipt. At the five-year point, Project Redirection achieved employment and welfare impacts, but did not affect educational progress. Nonetheless, over half of the treatment group members who received AFDC at baseline were also receiving it during the fifth year of follow-up.

5. Demonstrations have not been successful in delaying repeat childbearing among young women who have already had children. In JOBSTART, the Teenage Parent Demonstration, and Project Redirection, young mothers in the experimental group had rates of repeat pregnancies and births that were identical to or higher than those of young women in the control group. Why this should be the case is not clear, but there is qualitative evidence that, for many young women, being a mother was a source of gratification and self-esteem.

---

[21] Multi-component welfare-to-work programs for adult AFDC recipients have also shown considerably higher rates of participation in the first component than in subsequent activities (Gueron and Pauly, 1991).

6. *Programs have generally been less successful with school dropouts than with young mothers who were still in school.* Thirteen percent of LEAP teens qualified for four or more sanctions (mostly resulting in grant reductions) and no bonuses during the first 18 months of follow-up, and this group consisted largely of teens who had dropped out of school more than a year prior to entering LEAP. In Project Redirection, at the five-year point, women in the treatment group who were high school dropouts at program enrollment – unlike those who remained in school – were not better off than women in the comparison group in terms of employment or welfare receipt. High school dropouts in the Teenage Parent Demonstration registered significant increases in employment and decreases in welfare payments, relative to their control counterparts, but the program had no effect on earnings – a pattern that suggests that those experimentals who found employment also experienced rapid job turnover.

7. *Long-term follow-up is critical to determining the effectiveness of programs designed for young people, who may be better able to capitalize on what they have learned in these programs after they have gained greater maturity and their lives have become more stable.* The evaluation results of Project Redirection show the importance of such follow-up. In that demonstration, outcomes for program enrollees and members of a comparison group were disappointingly similar at 24 months after entry into the research. But at the five-year point, when most study sample members were in their early twenties, there was considerable evidence that Project Redirection had made a difference: Project Redirection enrollees were working more hours a week and had higher weekly earnings, they were less likely to be on welfare, they had created better home environments for their children, and their children showed better cognitive skills and fewer behavioral problems. Nonetheless, most young women in both research groups remained poor and on welfare.

VII. The Contents and Organization of This Report

The contents of this report pertain to all three components of the New Chance evaluation: early program impacts, derived from the 18-month survey; updated information on program implementation; and data on program costs.[22] The rest of the report is divided into eight chapters. Chapter 2 discusses the research design and associated issues and examines the characteristics of the research sample. Chapter 3 considers a number of topics related to the implementation of New Chance, focusing especially on the program activities, the extent to which experimentals participated in them, and their cost. The five chapters that follow present the 18-month impacts of New Chance on various measures of interest: service receipt by members of both the experimental and the control groups (Chapter 4); educational status (Chapter 5); fertility, health, and well-being (Chapter 6); parenting, child care, and child health (Chapter 7); and employment and welfare receipt (Chapter 8). Chapter 9 discusses the key themes and issues that emerge from this report and their implications for policymakers and practitioners.

---

[22] The full benefit-cost analysis awaits collection and analysis of the 42-month survey data (the principal source of information on program benefits) and will appear in the evaluation's final report, to be completed in 1996.

CHAPTER 2

RESEARCH DESIGN, SAMPLES, DATA SOURCES, AND ANALYSIS PLAN

I. Introduction

This chapter establishes the framework for the report's evaluation plan. It begins with a conceptual model of how New Chance was hypothesized to produce impacts, and then presents the research design, the study sample and its characteristics, and the data sources used for this report. The chapter concludes by discussing how the impact analysis is structured.

II. New Chance Participation and Impacts: A Conceptual Model

The comprehensiveness of the New Chance model reflects the expectation that the young mothers eligible for New Chance services faced multiple barriers to achieving self-sufficiency and personal growth. It also reflects the fact that the program sought to affect a broad range of short-term and longer-term outcomes for both the women participating in the program and their children.

Figure 2.1 presents a broad conceptual model that summarizes the hypothesized manner in which the program would yield impacts. A few caveats should be noted. First, the model specifies (in the various boxes) only those factors that have been measured as part of the research effort, and thus is necessarily incomplete. Many factors that affect the course of a young mother's life could not be captured adequately; nor, for the most part, could the program address them. In particular, it is recognized that the young mothers lived in complex family situations and community environments that were powerful forces in their lives. These (mostly unmeasured) contextual influences are acknowledged at the bottom of the figure.[1] A second caveat is that the model shown in Figure 2.1 is not intended to illustrate a path model that will be tested. Also, for simplicity's sake, the figure does not illustrate all the possible feedback loops among the various factors shown. For example, a child's characteristics affect a mother's parenting behavior, and that parenting behavior in turn affects the child's characteristics. The figure seeks primarily to identify key elements in a hypothesized causal chain.

In this figure, the various "columns" correspond to major points of data collection. Background characteristics of the mother and (to a very limited extent) the child were measured when the young mothers applied to the program. Program inputs were measured throughout the operation of New Chance. Short-term (18-month) outcomes were obtained during in-person interviews with mothers approximately 18 months after they applied to the program, and longer-term (42-month) outcomes are being captured in a second follow-up interview.

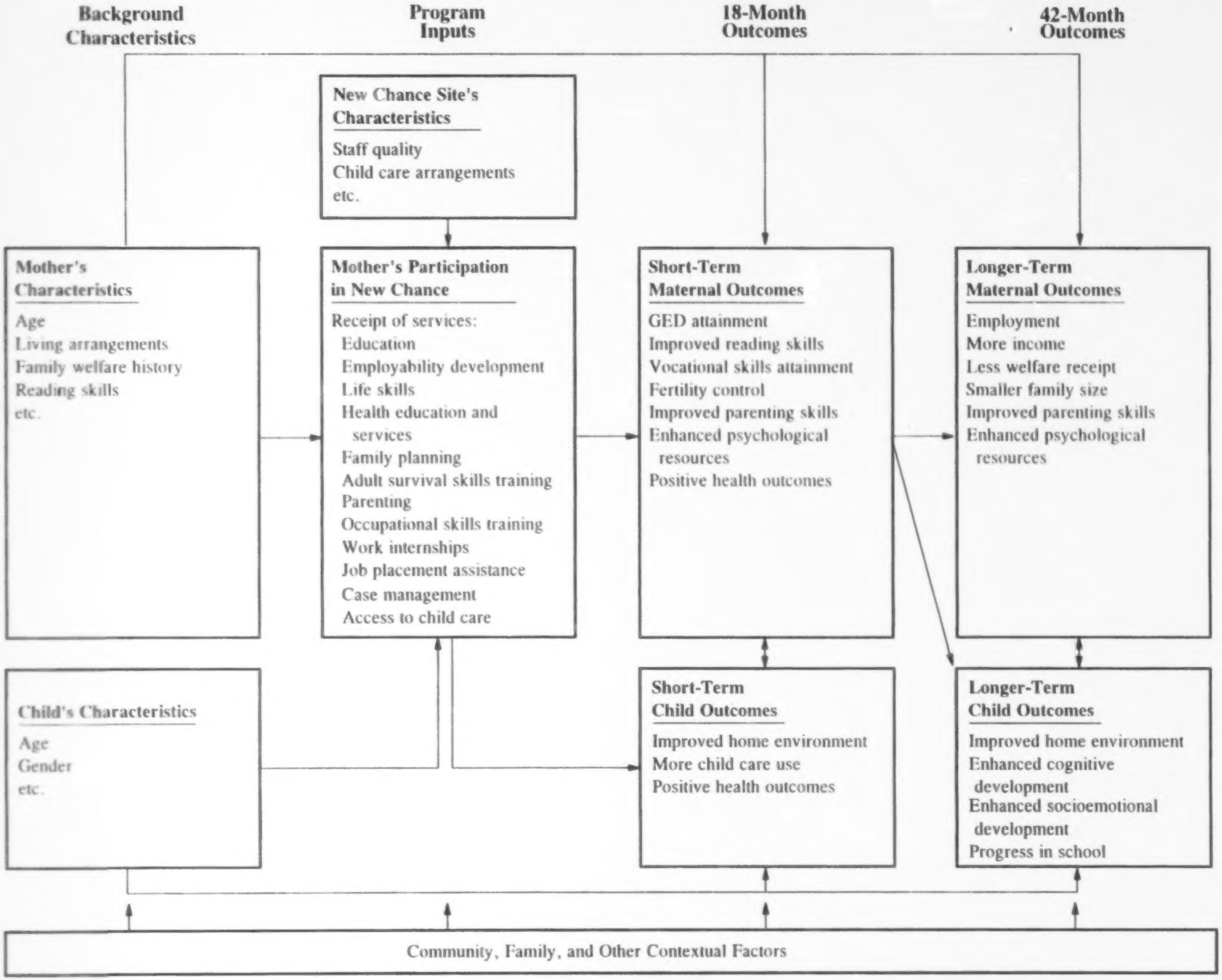
According to this model, the young women's background characteristics were hypothesized to influence the extent of their participation in New Chance activities. For example, young women who

---

[1] Examples include family characteristics, the local labor market, neighborhood characteristics, welfare rules, and the availability of social services other than New Chance.

FIGURE 2.1

A SIMPLIFIED CONCEPTUAL MODEL OF THE EFFECTS OF NEW CHANCE


Background Characteristics
Program Inputs
18-Month Outcomes
42-Month Outcomes
New Chance Site's Characteristics
Staff quality
Child care arrangements
etc.
Mother's Characteristics
Age
Living arrangements
Family welfare history
Reading skills
etc.
Mother's Participation in New Chance
Receipt of services:
Education
Employability development
Life skills
Health education and services
Family planning
Adult survival skills training
Parenting
Occupational skills training
Work internships
Job placement assistance
Case management
Access to child care
Short-Term Maternal Outcomes
GED attainment
Improved reading skills
Vocational skills attainment
Fertility control
Improved parenting skills
Enhanced psychological resources
Positive health outcomes
Longer-Term Maternal Outcomes
Employment
More income
Less welfare receipt
Smaller family size
Improved parenting skills
Enhanced psychological resources
Child's Characteristics
Age
Gender
etc.
Short-Term Child Outcomes
Improved home environment
More child care use
Positive health outcomes
Longer-Term Child Outcomes
Improved home environment
Enhanced cognitive development
Enhanced socioemotional development
Progress in school
Community, Family, and Other Contextual Factors

were quite depressed at baseline[2] may have been expected to participate less than women who were not depressed. Participation was also expected to be affected by characteristics of the site, such as staff quality, the presence or absence of on-site child care, etc.

The young mothers' participation in New Chance, in turn, was expected to have short-term impacts on a broad range of outcomes for both the mothers and their children. The model also acknowledges that, independent of program participation, the initial characteristics of the mothers and their children would influence both short-term and longer-term outcomes. For example, women with good reading skills at the outset might be more likely than poor readers to acquire a GED and to have better employment options.

Each New Chance program component was implemented with the intent of affecting certain outcomes. For example, participation in education classes was expected to increase reading skills and educational (especially GED) attainment. Through workshops and counseling on family planning, the young women were expected to enhance their use of contraceptives and their fertility control. Through parenting workshops, they were expected to improve their parenting skills and their ability to foster a favorable home environment for their children. Short-term impacts were, in turn, expected to affect favorably longer-term outcomes. According to the model, the gains a young woman realized while in the program – e.g., increased educational and vocational skills – would, over time, result in a greater likelihood that she would be employed and a smaller likelihood that she would receive welfare.

The developmental outcomes for participants' children were expected to be positively affected by New Chance, both directly through services provided to them and indirectly through effects on their mothers. It was expected that, in the short run, the mothers' participation in New Chance would have favorable effects on the children's home environment and health. The mothers' participation was also expected to alter their children's experiences with high-quality, non-maternal child care. It was hypothesized that, in the longer run, these short-term effects – as well as the hypothesized effects on family income and the mothers' educational attainment – would improve the children's cognitive and socioemotional development and their progress in school.

In summary, the framework presented in Figure 2.1 shows that several factors working together were expected to affect the long-term outcomes for young mothers: their initial level of resources and characteristics, the quality of New Chance services, the women's level of participation in those services, and their success in achieving the program's short-term goals. Variations in these factors may help explain why New Chance produced impacts for some, but not all, people in some, but not all, sites. Efforts to analyze the separate influence of these factors are discussed in the last section of this chapter.

---

[2] "Baseline" refers to the point when background information on sample members was collected, just prior to their being randomly assigned to the experimental or control group. It also marks the starting point for the follow-up period. Thus, e.g., the phrases "characteristics at random assignment" and "characteristics at baseline" or "baseline characteristics" are synonymous and are used interchangeably in this report.

III. The Study Design

A. Principal Questions

The conceptual framework presented in the previous section suggests that the most appropriate focus for this report is on the short-term impacts of the program – on those domains in which one might expect to find a near-term difference in outcomes as the direct result of New Chance participation. Thus, the data collection and analysis strategies were designed primarily to address questions as to whether New Chance increased the use of human capital development (i.e., education and training) and other services, improved enrollees' educational attainment and achievement, increased effective use of contraception and resulted in fewer pregnancies, improved the young mothers' psychological well-being, improved their parenting skills, and affected their use of child care. Two issues in particular – the program's impacts on GED attainment and fertility control – receive special attention in the analysis because they were expected to have an especially powerful effect on the young mothers' ability to attain self-sufficiency in the longer term.

Longer-range outcomes – such as employment and earnings, welfare receipt, and income – are included in this report in the interest of completeness, but with the recognition that it will take longer for expected program impacts in these areas to become manifest. Capturing the program's longer-term effects – especially on the mothers' economic well-being and their children's development – is central to ongoing data collection efforts. The results will be presented in the project's final (1996) report, based on interviews and assessments completed approximately 42 months after random assignment.

B. The Random Assignment Strategy

The New Chance Demonstration used random assignment, a lottery-like procedure, to divide New Chance applicants into two groups: an experimental group and a control group. While members of the experimental group were offered access to New Chance, control group members were excluded from the program, though they were free to seek services elsewhere in the community. Random assignment ensured that, aside from random variation in individual characteristics, experimentals and controls were similar in all other respects; the only systematic difference between the two research groups was their access to the New Chance program.[3] As a result, differences in program outcomes between the two groups (such as educational attainment) can be attributed to New Chance. Outcomes for the control group presumably capture what would have happened to those in the program had it not been offered to them. A control group created through random assignment is a valid reference point against which the experiences of experimentals may be compared.

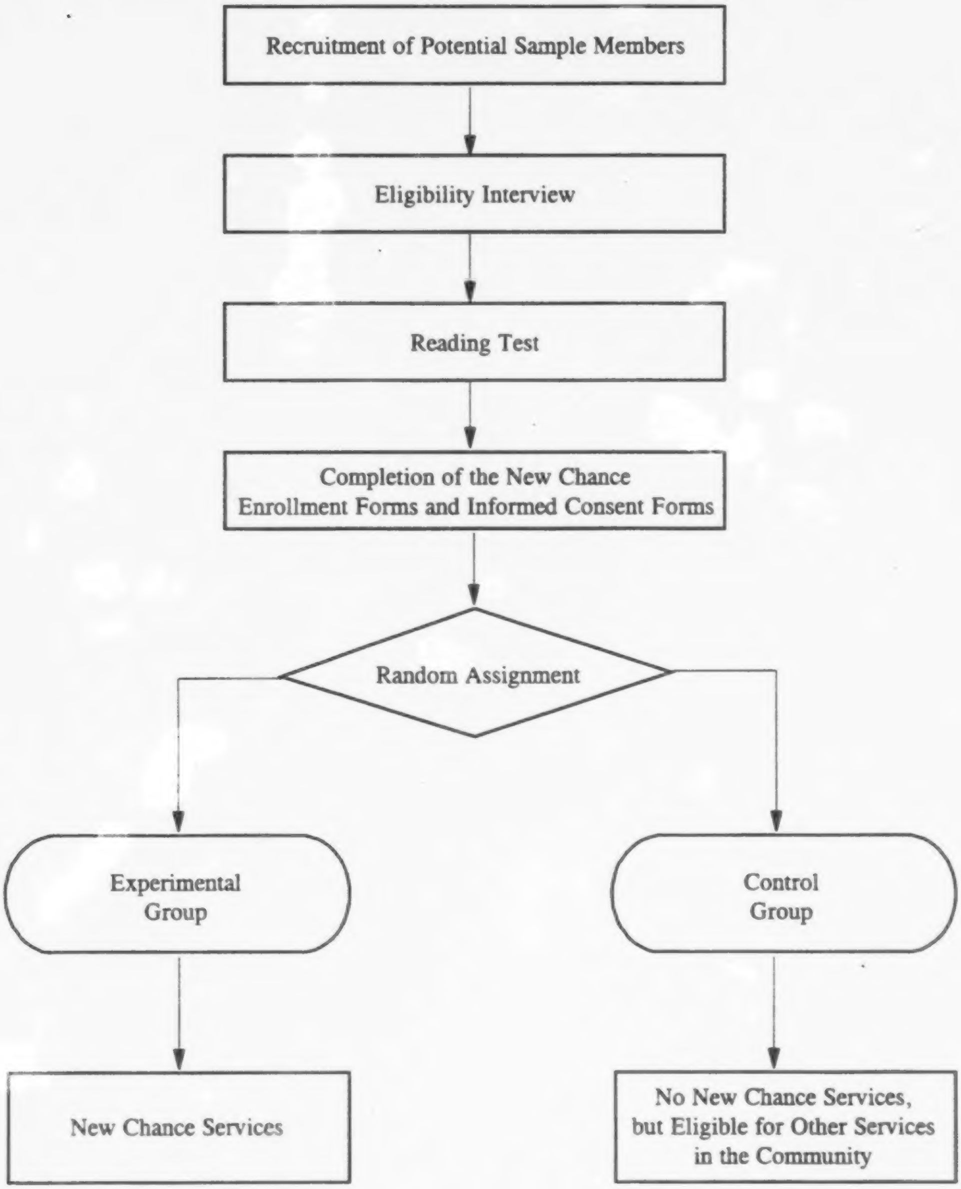
Entrance into the study sample occurred between August 1989 and July 1991. Figure 2.2 shows the steps in building the sample. Program guidelines called on each site to recruit 150 eligible young mothers, of whom 100 were to be assigned to the experimental group and 50 to the control group. (A two-to-one random assignment ratio was adopted because it was reasoned that sites would respond

---

[3] The experimental design was statistically tested to ensure that the two groups created by the random assignment process were indeed similar. No systematic differences between them were found (see Appendix A for details).

FIGURE 2.2

STEPS IN THE INTAKE AND RANDOM ASSIGNMENT OF THE NEW CHANCE RESEARCH SAMPLE


Recruitment of Potential Sample Members
Eligibility Interview
Reading Test
Completion of the New Chance Enrollment Forms and Informed Consent Forms
Random Assignment
Experimental Group
Control Group
New Chance Services
No New Chance Services, but Eligible for Other Services in the Community

more favorably to random assignment and probably would be able to recruit applicants more readily if young women knew that their chance of admission to the program was better than 50-50.) Eligibility for the program was determined through an initial interview, during which data on the young women's characteristics at sample entry were recorded on the New Chance Enrollment Form; a reading test was also administered to program applicants.[4] As part of the application process, applicants signed an informed consent statement, acknowledging their participation in the study and allowing MDRC researchers to collect data on them.

After a young woman's paperwork was completed, a member of the local program's staff called an MDRC random assignment clerk, who used a computer program to generate an assignment to the experimental or control group. The experimentals were offered entry into the New Chance program; controls were denied entry into New Chance but were given a list of alternative services available in the community.

A total of 2,322 young women were randomly assigned: 1,553 to the experimental group and 769 to the control group. Members of both research groups were followed up through in-home interviews at 18 months after random assignment; these 18-month surveys were the source of the impact data presented in this report.

IV. The Research Sample

A. Samples Used in This Report

As is usually the case with survey data, not all 2,322 young women who were randomly assigned could be located or were willing to respond to the 18-month survey. Nevertheless, the response rate was high: 91.4 percent for experimentals and 89.3 percent for controls. In addition to sample members who did not respond at all, some respondents failed to answer certain questions in the survey or gave invalid answers.[5] Also, in those cases where the survey was not administered in the respondent's home, certain interviewer observations could not be made. Overall, valid data on most outcomes were available for 2,088 (99.1 percent) of the 2,106 survey respondents. This sample of 2,088 survey respondents was the sample used for most of the outcomes presented in this report: receipt of education, skills training, and other program services (Chapter 4); educational attainment and achievement (Chapter 5); living arrangements (Chapter 6); and employment, family income, and welfare (Chapter 8). Other outcomes are covered by smaller subsamples of the overall (2,106-member) survey respondent sample, as follows:

---

[4] The test used at most sites was a short form of the reading part of the Tests of Adult Basic Education (TABE), although a few sites used the long form instead. For program applicants who were high school graduates, the TABE score was used to confirm that they read below the ninth-grade level, a condition of program eligibility for high school graduates (as noted in Chapter 1). For others, the TABE provided baseline information on reading skills that would be useful to the evaluation.

[5] "Invalid answers" include inconsistencies in the answers to related questions on the survey and out-of-range values such as "February 31."

	Sample Size
Participation outcomes (Chapter 3)—Experimentals only	1,408
TABE literacy scores (Chapter 5)	2,046
Pregnancy, birth, and health outcomes (Chapter 6)	2,024
Parenting and home environment outcomes (Chapter 7)	1,842
Child care outcomes (Chapter 7)	1,932

Unless otherwise stated, whenever mention is made of "the sample" in the remaining chapters, it refers to the relevant sample or subsample noted above.

Appendix B displays selected baseline characteristics for most of these subsamples and for the full New Chance study sample of 2,322 young women. It also presents the results of statistical tests that explored the statistical significance of differences in these characteristics across the samples. In brief, these tests suggested that no large and systematic differences exist between the survey respondent samples and the full New Chance study sample.

B. Sample Characteristics

Table 2.1 provides detailed background information on the 2,088 survey respondents who constituted the sample for most of this report. The data were based on the Enrollment Form completed when sample members applied for entry into the program.

The table shows that most of the young women in the sample could be considered truly disadvantaged at the time of their application to the program. Consistent with the effort to target welfare recipients and dropouts, 94.8 percent of all sample members were receiving AFDC at baseline, and almost 94 percent had not graduated from high school or earned a GED. More than half the sample members (63.3 percent) had not worked at all during the 12 months prior to applying for New Chance. Of those who worked, the majority (69.7 percent) earned $1,000 or less during this period (not shown in the table).

Sample members' average age at the time of application was 18.8. The youngest sample members were 16 years old, while the oldest were 22. Most young mothers in the sample (64.9 percent) had one child, and for most sample members (53.8 percent), the youngest child was less than a year old. Twenty-nine percent of all sample members reported not using contraception the last time they had intercourse, placing them at risk for an additional pregnancy.

The large majority (90.1 percent) of the sample had never been married before their application to New Chance. Of those who had been married, only three in ten (2.9 percent of the entire sample) were still living with their spouse at baseline. A total of 11.6 percent of the sample reported living with a husband or partner at the time of random assignment (not shown in the table).

About a third of all sample members (34.4 percent) were living with their mothers, and 31.8 percent were living in a household of their own with no other adult present. Because of the significant policy interest in requiring young mothers on welfare to live with their own mothers or with other adults, the baseline characteristics of both young women who lived with their mothers and those who

TABLE 2.1

SELECTED CHARACTERISTICS OF THE NEW CHANCE SAMPLE AT RANDOM ASSIGNMENT

Characteristic and Subgroup at Random Assignment	Average or Percent
Demographic characteristics	
Age (years) (%)	
16	2.1
17	17.5
18	22.1
19	25.6
20	19.5
21	11.1
22	2.1
Average age (years)	18.8
Ethnicity (%)	
Black, non-Hispanic	52.4
Hispanic	22.3
White	22.7
Other	2.5
Marital status (%)	
Never married	90.1
Other	9.9
Number of children (%)	
1	64.9
2	26.8
3 or more	8.3
Average number of children	1.4
Age of youngest child (years) (%)	
Less than 1	53.8
1	26.6
2	12.1
3 or older	7.5
Average age of youngest child (years)	1.2
Age at first child's birth (years) (%)	
14 or under	5.3
15	12.1
16	22.9
17	26.1
18	21.6
19	12.1
Average age at first child's birth (years)	16.8
Living arrangement	
Living with (%)	
Mother	34.4
Father	7.6
Spouse or partner	11.6
No other adult	31.8

(continued)

TABLE 2.1 (continued)

Characteristic and Subgroup at Random Assignment	Average or Percent
Lived in a female-headed household at age 14 (%)	48.9
Lived with both parents at age 14 (%)	22.2
Education characteristics	
Highest grade completed (%)	
7th or below	3.0
8th	10.3
9th	22.8
10th	30.3
11th	27.8
12th	5.8
Average highest grade completed	9.9
Received high school diploma or GED (%)	6.3
Left school before first pregnancy (%)	37.2
Average number of years since last attended school	2.4
Reading level (grade equivalent) (%)	
4th grade or below	8.5
5th grade	5.8
6th grade	9.4
7th grade	10.8
8th grade	14.0
9th grade	21.4
10th grade or above	30.1
Average reading level (grade equivalent)	8.4
Desired educational attainment for self (%)	
High school diploma or GED	32.6
1-3 years of college (A.A. degree)	31.2
4 years of college (B.A. degree)	22.2
Graduate degree	10.8
Other	3.2
Desired educational attainment for child (a) (%)	
Elementary school	0.2
High school	20.8
College/post-secondary	57.8
Graduate school	21.2
Mother has high school diploma or GED (%)	52.5
Mother attended college (%)	25.1
Father has high school diploma or GED (%)	43.1
Father attended college (%)	16.2
Both parents have high school diplomas or GEDs (%)	29.4
Both parents attended college (%)	7.3

(continued)

TABLE 2.1 (continued)

Characteristic and Subgroup at Random Assignment	Average or Percent
Employment and welfare receipt	
Number of jobs ever held (%)	
0	21.5
1–2	32.7
3 or more	45.8
Average number of jobs held	4.1
Employed at random assignment (%)	3.1
Number of months employed in prior 12 months (%)	
0	63.3
3 or less	18.4
4–6	10.0
7–12	8.0
Prior–year earnings (%)	
$0–$500	79.9
$501 or more	20.1
Length of longest job (%)	
Never employed	21.2
Less than 1 month	3.7
1–3 months	22.7
4–6 months	22.3
7–12 months	17.9
Over 1 year	12.2
Mother employed (%)	
Yes	49.8
No	42.2
Don't know	4.0
Deceased	4.0
Father employed (%)	
Yes	45.4
No	19.8
Don't know	25.0
Deceased	9.8
Receives AFDC (%)	
Own grant	87.4
Other person's grant	7.4
Not receiving AFDC	5.2
Receives (%)	
Medicaid	87.1
Food stamps	83.7
Public housing	23.2
Income from a job	3.5
Family received AFDC when sample member was growing up (%)	
Always	16.6
2 years or less (b)	18.7
More than 2 years, but not always (b)	28.5
Never	36.2

(continued)

TABLE 2.1 (continued)

Characteristic and Subgroup at Random Assignment	Average or Percent
Fertility-related characteristics	
Number of pregnancies (%)	
1	43.4
2	32.2
3	16.1
4	6.0
5 or more	2.4
Average number of pregnancies	1.9
Ever had an abortion (%)	23.1
When next child is expected (%)	
Not expecting another child	64.2
Within 2 years	7.0
In 2-4 years	11.6
In 5 years or more	17.3
Average number of years until next child is expected (c)	4.4
Current birth control use (%)	
Yes, using birth control	62.4
No, not using birth control	12.1
No partner/not having sex	25.5
Used birth control at last intercourse (%)	71.0
Relations with child's father	
Speaks with child's father (a) (%)	67.5
Has child support order (a) (%)	27.9
Prior and current service receipt	
Ever in occupational skills training (%)	22.3
Services received in the 60 days before random assignment (%)	
Health care for child	84.6
Family planning	22.9
Mental health	2.7
Health care for self	58.7
Parenting	11.0
Life skills	2.9
Counseling	4.1
Other services	10.5
No services	8.4
Has regular child care (d) (%)	43.7
Psychosocial characteristics	
CES-D (depression) Scale (e) (%)	
0-15 (not at risk)	47.0
16-23 (at some risk)	25.9
24-60 (at high risk)	27.2
Average CES-D score (e)	18.1

(continued)

TABLE 2.1 (continued)

Characteristic and Subgroup at Random Assignment	Average or Percent
Average number of sources of emotional support	2.7
Average level of satisfaction with emotional support (f)	4.2
Average self–esteem score (g)	38.3
Average Locus of Control score (h)	22.0
Other	
Has home telephone (%)	83.9
Has driver's license (%)	27.9
Sample size	2,088

SOURCES: MDRC calculations from New Chance Enrollment Form data.

NOTES: Calculations for this table used data for all 2,088 sample members for whom there were 18 months of follow–up survey data, including those with values of zero for outcomes and New Chance enrollees (i.e., experimentals) who did not participate in the program. The reported sample sizes may fall short of this number because of missing or unusable items from some sample members' questionnaires.

Distributions may not total 100.0 percent because of rounding.

(a) When a sample member had more than one child, her response refers to her first child.

(b) The family's AFDC receipt may not have been continuous.

(c) Includes only those sample members who expected to have more children.

(d) Regular child care was defined as an ongoing arrangement used while the mother was in school, in training, or working.

(e) The Center for Epidemiological Studies Depression (CES–D) Scale is a widely used measure of depression; scores can range from zero to 60.

(f) Enrollees were also asked about their degree of satisfaction with the emotional support ("people who listen to you, reassure you, and show you they care") they received. Levels range from 1 (very dissatisfied) to 5 (very satisfied).

(g) The measure of self–esteem used was the Rosenberg Self–Esteem Scale, a 10–item scale that assesses a person's global sense of self–worth. Scores can range from 10 to 50; 30 is considered the neutral midpoint.

(h) The Locus of Control Scale is a six–item adaptation of the longer scale originally developed by Julien Rotter (1966). Scores can range from 6 to 30; 18 is considered the neutral midpoint.

had other living arrangements were examined. As expected, the two groups differed in many respects.[6]

As part of the enrollment process, the young women who applied for New Chance were also asked questions regarding their psychological status and the degree of social support they experienced.[7] Depression was measured with the Center for Epidemiological Studies Depression (CES-D) Scale. The table shows that 53.1 percent of all sample members registered scores of 16 or higher, generally considered to place them at risk for a clinical diagnosis of depression; 27.2 percent had scores of 24 or higher, indicative of high risk of such a diagnosis. However, scores on scales of self-esteem and social support do not appear to be particularly low, and the young women in the sample recorded a higher-than-expected score on a nationally used scale measuring one's sense of control over one's life. This measure may have captured their positive expectations about entering this program.

By design, the New Chance sample was homogeneous with regard to many key characteristics: All sample members were recruited from a group of disadvantaged young mothers with similar childbearing and education histories. However, there was still considerable variation in the sample. For example, at program entry, 14.3 percent read below the sixth-grade level, but almost a third (30.1 percent) read at the tenth-grade level or higher. And while 16.6 percent came from families that had always received welfare when they were growing up, 36.2 percent came from families that had never received AFDC.

Variation in sample members' characteristics was especially pronounced across the New Chance sites.[8] Site-specific entry criteria were one source of such variation. Thus, e.g., the Minneapolis site required that all program applicants read at the sixth-grade level or higher. This helps to account for the fact that the average reading level at baseline in Minneapolis was the highest in the demonstration. Other differences are explained by underlying differences in the target population. For example, sites such as the Bronx, Chula Vista, Denver, and San Jose had relatively large numbers of Hispanic enrollees, while the majority of sample members in Portland and Salem were white. Other site differences are less readily explicable – for instance, the fact that only in Lexington did the majority of sample members have more than one child.

---

[6] Those not living with their mothers were older and more likely to be white. More of them had been married, and they had had more pregnancies; a higher proportion also had two or more children. Those not living with their mothers were less likely to have lived with both parents at age 14 and more likely to have held a job. They were more likely to be receiving AFDC on their own case, and less likely to be receiving AFDC on someone else's case; they were also more likely to be receiving food stamps or Medicaid, or to live in public housing. Young mothers who did not live with their own mothers expected to have fewer additional children and were more likely to be using birth control. Young women living apart from their mothers were also less likely to have used child care, less likely to have a phone, and more likely to have a driver's license. There were no significant differences at baseline between those living with their mothers and those living in other arrangements with regard to the average highest school grade completed, reading level, educational aspirations, family receipt of welfare during childhood, depression, and the young women's age at first birth.

[7] For a discussion of the measures and scales that were used, see Quint, Fink, and Rowser, 1991, pp. 90-91.

[8] An extensive table with sample characteristics by site is included in this report as Appendix E. This table also presents the results of tests of the statistical significance of cross-site differences. Virtually all cross-site differences were statistically significant.

Portraits in Diversity

"Young mothers on welfare." The phrase conjures up stereotypes that belie tremendous diversity in the situations of these young women and their children, as the following profiles, taken from the New Chance monograph (Quint and Musick, 1994), suggest.

Patricia lives with her two children, her boyfriend (the father of her younger child), and her boyfriend's family in a nicely furnished house in a pleasant neighborhood. Her boyfriend's parents both work, and Patricia, whose younger child is five months old and nursing, stays at home. She does not pay rent, but she does do the housework, and the place is immaculate. She receives AFDC for herself and her older child (but not for her younger one) and augments her grant by occasionally babysitting for neighborhood children.

Patricia's mother works for a large corporation (her father died a few years ago), and her sister is a college graduate. Patricia, however, disliked high school, and her mother gave her a choice: Stay in school or go to work. Patricia worked for a while as a sales clerk in a shoestore and moved out of her mother's home. Subsequently, she became pregnant with her first child.

Patricia enrolled in New Chance, where she received her GED, and then entered a community college. She lasted only a semester, dropping out both because she was pregnant and because the courses were too difficult. She wants to stay at home until her baby is a year old, but she's not sure what she wants to do afterward – return to college or get a job. She and her boyfriend talk about getting married and moving into a place of their own. But right now all plans are on hold: He was laid off from his job, and any money he earns from odd jobs goes toward fixing their car. Recently she urged him to apply for AFDC as an unemployed father so that their child would qualify for Medicaid coverage.

Letrice's mother was an alcoholic who refused to touch or embrace her daughter, claiming that doing so was physically painful; and Letrice went to live with her grandmother. The ailing woman had difficulty controlling her granddaughter, and Letrice had been placed in foster care several times by the time she was 14. A psychological evaluation conducted at that time indicated that she had feelings of helplessness and low self-esteem; also of concern to the child welfare authorities were Letrice's shoplifting, her experimentation with alcohol and marijuana, and her frequent truancy. The evaluation noted that Letrice lived in a crime- and drug-ridden neighborhood, and that an older brother had also used drugs. Letrice was committed as a status offender to foster care until she was 19. Her daughter was born when she was 17, and Letrice enrolled in New Chance shortly thereafter.

While attending New Chance, she had a particularly difficult time with one foster care provider. She left her daughter in the provider's care while she looked for another place to live, staying in a homeless shelter until she could find someone else who was willing to take her child in along with her. The foster care provider sued to retain custody of Letrice's baby, claiming that she was a neglectful mother. With the help of New Chance staff, the case was resolved in Letrice's favor, and her daughter was restored to her custody.

Life since then has not been easier. Letrice has held a series of part-time jobs with two fast-food restaurants, a cleaning service, and a movie theater. Letrice and a friend decided to share an apartment, but after her friend defaulted on the rent, Letrice moved in with her mother. She was grateful for a place to stay, but the house was unheated and the food inadequate. Letrice moved from there into a Salvation Army shelter for about a week and eventually found a low-income apartment that would be pleasant were it not roach-infested. Recently, Letrice's brother, who was visiting her after having been released from jail, was drinking heavily during a card game, grew angry at her, and beat her with a kitchen chair, breaking two of her fingers.

Despite this variation, sample members' baseline characteristics show that New Chance reached the intended target population of disadvantaged young mothers. At baseline, the majority of the sample had no education credentials, very little work experience, and limited reading skills. Most young women in the sample were taking care of very young children, so child care and other ancillary services were needed to facilitate their participation in education and training activities. The high level of depression also suggests that the program reached young women experiencing emotional problems as well as practical ones.

V. Data Sources for This Report

This report drew on several kinds of information. As noted earlier, the source of data on program impacts (Chapters 4 through 8) was the survey that was administered to experimentals and controls at approximately 18 months after random assignment.[9] The 18-month point was chosen for the first round of interviews because enrollees were allowed to remain in New Chance for up to 18 months. The 18-month survey was designed to measure the short-term effects of the program, with particular emphasis on the mothers' education, fertility, parenting, and use of child care.

Table 2.2 lists the various parts (or modules) of the survey, along with a rationale for their inclusion. The survey was conducted in person, almost always at the sample member's home, and took just under 90 minutes, on average, to complete. In assessing parenting behavior, sample members' answers were supplemented by the interviewer's observations. To curtail the survey's average length, some questions were asked of only half the sample, selected at random.[10]

The impact analysis also relied on the Enrollment Form and baseline reading test that were completed by each sample member prior to random assignment. The form included information about her prior education, training, and work experience; welfare history; family composition; living arrangements; and psychological well-being. Since all these items were collected before random assignment, they are fully independent of the sample members' research status (i.e., membership in the experimental or control group). Thus, baseline data can be used to define subgroups of experimentals and controls for which experimental impact estimates can be generated.[11]

The source for Chapter 3's data on experimentals' participation in New Chance and duration of activity in program components was the New Chance Management Information System (MIS). For

---

[9] Administrative records could have been used to measure earnings, employment, and welfare outcomes, but they would have been of little use in assessing program effects on fertility behavior, education gains, parenting, and child outcomes.

[10] Outcomes based on these questions thus had smaller sample sizes, reducing the precision of the impact estimates. However, since the potential respondents were selected at random, these impacts should not be biased (i.e., they should remain representative of the full New Chance research sample).

[11] Up to 51 of these baseline items were also included as covariates in impact regressions, thereby improving the precision of the impact estimates. Reported average outcomes were *regression adjusted* using one-way or two-way analysis of covariance (see Ostle, 1975, p. 461; Cave, 1987). This means that control variables were included to remove slight imbalances in baseline characteristics between the experimental and control groups as a cause of variation in the outcomes.

TABLE 2.2

NEW CHANCE 18-MONTH FOLLOW-UP SURVEY MODULES

Module	Rationale
Participation in education and training activities	To describe differences in the receipt of education and training services by experimentals and controls.
Participation in other services	To describe experimental-control differences in the receipt of non-education services.
Educational achievement and attainment	To measure literacy outcomes and receipt of education credentials.
Household structure, marriage, and residence	To describe changes in living arrangements as a possible outcome of participation in New Chance.
Fertility and family planning	To measure the impact of special services targeted to changing family planning behavior and increasing the use of birth control.
Psychological variables	To measure program effects on indicators of emotional well-being such as depression, self-efficacy, and access to support.
Health and health care	To measure program impacts on health status, access to health care, and health care utilization.
Employment	To measure short-term employment and earnings outcomes, as well as job-seeking behavior.
Income and welfare receipt	To capture program effects on the amount of family income, the combination of income sources, and receipt of public assistance.
Parenting and home environment	To measure program effects on parental behavior and attitudes, and on the home environment of children.
Child care	To describe program effects on child care arrangements.
Variables related to child's father	To capture program effects on the relationships of the children of sample members with their biological fathers or with other father figures.

SOURCE: New Chance 18-month follow-up survey.

each New Chance enrollee, site staff completed and sent to MDRC a monthly time sheet, which recorded the number of days she attended and the number of hours she participated in each program component. Data for the cost analysis (Chapter 3) were gathered by MDRC staff, who received detailed cost reports from the New Chance sites and conducted time studies to allocate these costs to the various program components.

Data on program operations were gathered from several sources. MDRC researchers conducted "wrap-up" interviews with program coordinators to ascertain their views, usually based on several years' experience, on the key issues involved in running the program and on ways they would change the program model or its implementation. MDRC staff also completed reports on the sites' experiences in operating the program components. Using widely accepted rating instruments, MDRC staff also conducted assessments of the quality of child care provided by the New Chance sites. Finally, the 18-month survey elicited experimentals' reactions to the program and its components – information that is drawn on in several chapters of the report.

VI. The Structure of the Analysis

A. Aggregate Impacts

In this report, all impacts are first presented for the sample as a whole. These *aggregate* impacts represent the average difference between the experiences of *all* experimental group members and *all* control group members.[12] Included in the calculations were experimentals who dropped out of New Chance soon after random assignment or who chose not to participate at all, as well as those who received substantial amounts of program services. Also included were controls who found and received alternative services outside the New Chance program. Thus, strictly speaking, the impacts represent the effects of the *additional* services New Chance provided above and beyond what control group members received on their own.[13]

For each sample member, the first of the 18 months of follow-up was usually the month in which she was randomly assigned to the experimental or control group. Consequently, the number of days of follow-up in that first month varied depending on the day of the month the sample member was randomly assigned.

B. Impacts for Subgroups and Sites

Average impacts for an entire sample often encompass a good deal of variation, some of which is attributable to differences in the characteristics of sample members and of program sites. To "get behind" the averages, and to find out how particular groups fared (e.g., mothers who were 16 or 17 at random assignment versus those who were older, women who had one child at that point versus those who had more, or those who had grown up in families that had always received AFDC versus those who had received it for briefer periods or not at all), impacts were also analyzed separately for a number of such "subgroups," which were defined by characteristics of the sample at random assignment (e.g., age, family welfare history, etc.).[14] Knowing what sorts of people the program did or did not benefit could have implications for targeting and designing programs and for developing effective public policy.[15]

---

[12] Similarly, results for subgroups and sites are based on responses for all experimentals and all controls within the subgroup or site.

[13] Most impacts were estimated using ordinary least square impact regressions. However, impacts on categorical variables (such as living arrangements or fertility behavior) were replicated using a multinomial logit technique, which takes account of the fact that different categories in the outcome variable are not independent. A Tobit estimator was used to estimate impacts on outcome variables that were truncated at the end of the follow-up period (such as "time until entered education activity"). This estimator uses the observed part of the distribution of such a truncated variable to approximate the unobserved tail of this distribution.

Similarly, knowing the results for individual sites could inform possible efforts to replicate New Chance as well as future directions at the sites themselves.[16] For a few particularly important impacts (e.g., service receipt and GED attainment), site variation is explored in some detail. In these cases, an attempt was made to isolate "true" site variation from variation that resulted simply because, as noted above, the characteristics of participants at the different sites also differed.[17]

C. Statistical Significance

As noted in Chapter 1, the concept of "statistical significance" refers to the idea that a measured difference between two or more groups on a given indicator is unlikely to have arisen simply by chance. In this report, following common conventions, an impact was considered to be statistically significant if there was a smaller than 10 percent probability that it was the result of mere random variation across individuals (i.e., chance). In the tables, statistically significant impacts are marked with asterisks: One asterisk represents a smaller than 10 percent probability that the finding arose by chance; two asterisks, a smaller than 5 percent probability; and three asterisks, a smaller than 1 percent probability. In addition to these asterisks, many impact tables in this report have columns showing "p-values." A p-value represents the probability that a finding is the result of random variation (i.e., chance). For example, a p-value of .20 represents a 20 percent chance that the finding it refers to arose by statistical chance. A p-value of .10, which corresponds to a single asterisk, indicates that the chance of such a spurious finding is 10 percent. P-values allow for a more extensive assessment of the reliability of the findings they refer to than do asterisks. For instance, an impact with a p-value of .12 would not be flagged with an asterisk, but may still be considered "marginally significant," since it comes close to .10, which *is* significant.

---

[14] Owing to smaller sample sizes, subgroup estimates are generally less reliable than estimates generated for the full sample. Subgroup breakdowns that were not included in any of the impact tables because they did not capture significant impact variation across any key outcomes included marital status at baseline, prior receipt of vocational training, and random assignment cohort (i.e., whether members entered the research sample relatively early or late during the random assignment period).

[15] Statistical tests are needed to establish whether or not subgroup and site differences are systematic or the result of random variation. These tests are based on the assumption that allowing the program effect to vary by subgroup or site leads to a better "fit" of the impact regression. If this improvement in "fit" is statistically significant, the subgroup variation is considered significant as well. The actual analysis is done as follows: Baseline variables capturing the subgroup and site differences are interacted with a dummy variable identifying experimental status. The single experimental dummy is augmented with these interacted program variables in the impact regression. A joint F-test comparing the interacted and uninteracted regression results is then used to establish the significance of the gain in explanatory power caused by this substitution, providing a measure of the statistical significance of the impact variation across subgroups. (See Kennedy, 1992, p. 57.)

[16] The smallest site enrolled only 69 sample members (experimentals and controls combined) and the largest, 171. Small sample sizes reduce the likelihood that even fairly large percentage differences will be statistically significant and, therefore, that the results can be ascribed to anything except chance.

[17] This was done by including all 36 non-site interactions of individual baseline characteristics with the experimental dummy in the regression equation containing the interacted site dummies. These added interactions removed any site-specific impact variation that was due to differences in individual sample characteristics, rather than being attributable to site-specific factors.

D. Nonexperimental Extensions of the Analysis

Most of the impact information presented in this report is based on experimental comparisons (i.e., the impacts are the difference in outcomes between experimentals and controls). In addition, however, some nonexperimental comparisons were made to clarify or disaggregate experimental results. Thus, e.g., while the experimental analysis determined the effect of the additional services received by experimentals, as compared to controls, there was also an interest in understanding how experimentals who received varying amounts of services fared. In Chapter 5, rates of GED attainment are compared for members of the experimental group who received many hours of New Chance services versus sample members who received few or no services. Other such nonexperimental comparisons are presented in Chapters 6 (family planning) and 7 (parenting).

Comparisons of this type involve subgroups of experimentals created using sample members' *post-baseline* experiences. Consequently, the results are subject to potential "selection bias."[18] Experimentals who received a lot of service might have been systematically different from experimentals who did not. Unless removed by statistical procedures, these differences may cause bias in estimates that are based on such post-baseline experiences. For several key analytical questions (such as the relationship between program intensity and educational attainment), statistical techniques were used in an attempt to assess the threat of selection bias.[19]

---

[18] The term *selection bias* refers to the notion that a selection process usually precedes a person's receipt of services. This selection process, be it an individual decision or an institutional procedure, tends to create systematic differences between those who receive services and those who do not, which interfere with the assessment of the effect of the services per se. Generally, these "pre-existing" differences are not limited to measurable characteristics, but include difficult-to-measure concepts such as motivation and aptitude.

[19] An example of such a nonexperimental approach is a multiple Mahalanobis matching strategy, which was used by Cave and Bos in their analysis of JOBSTART impacts (Cave and Bos, 1994). This strategy used baseline characteristics to match experimentals to their individual control group counterparts. If successful, this method would have shown little danger of selection bias in individual experimental-control comparisons, thus allowing a breakdown of the sample by post-random assignment experiences. However, when applied to several New Chance outcomes, this method failed a crucial specification test, suggesting that selection bias may be a real issue in nonexperimental analyses involving the New Chance sample. This test is based on the assumption that the impact of a voluntary program on those who fail to show up to receive services has to be zero, since the only way a voluntary program can affect someone is through the provision of actual services. A non-zero impact for this group after matching is completed implies that there are unmeasured differences between members of pairs that were matched using measured baseline characteristics. These unmeasured

(continued...)

An attempt was made to remove potential selection bias from these comparisons using an *instrumental variables* technique.[20] Implementation of the instrumental variable technique in the New Chance impact analysis did not uncover statistically significant relationships between the receipt of specific services and outcomes such as educational attainment, parenting, or fertility behavior.

---

[19](...continued)
differences cannot be controlled for statistically and threaten the validity of nonexperimental comparisons. See Cave and Bos, 1994, and Bloom, 1984.

[20] This technique includes data for experimentals and controls, and uses the experimental assignment dummy to remove selection bias from the service receipt variables. This is done as follows: Along with measured baseline characteristics, the experimental dummy is used in a first-stage regression to predict service receipt. Instead of actual service receipt, predicted values of service receipt are saved and used in a second stage to estimate effects on a criterion variable, such as GED attainment or fertility behavior. Because predicted values of service receipt are uncorrelated with the unexplained part of the criterion variable, selection bias is removed from the regression of the criterion variable on service receipt. The fact that the experimental dummy is highly correlated with the receipt of service, but uncorrelated with the error term in the first stage, improves the efficiency of this procedure. Such a "clean" predictor of service receipt is referred to as "the instrument" in the instrumental variables technique. Unfortunately, results from this procedure are generally imprecise. They are also based on the arguable assumption that *all* program impacts on educational attainment are achieved via education services, all program impacts on pregnancy are achieved via family planning services, etc.

CHAPTER 3

IMPLEMENTING NEW CHANCE

I. Introduction

An examination of program implementation – a topic that includes the structure and content of program components,[1] operating procedures and issues, and the extent of participation in program activities – is critical for several reasons.

First, the manner in which the program was implemented may account in part for why it did or did not produce certain impacts. Especially in a multi-site demonstration, site variation on important dimensions of implementation (e.g., service structure, participation level, and quality) may help to explain variation in impacts and costs as well. Second, the issues confronted in mounting a program and the level of effort required (along with the program's impacts and costs) are among the factors to be considered in deciding whether the intervention can be sustained and should be replicated.

Finally, it is important to consider whether the program was implemented in such a way that the impact analysis constitutes a fair test of the underlying model. Essentially, the model *did* receive a fair test, although, for reasons discussed below, participation in the Phase II components was lower than anticipated.

A. Data Sources and the Organization of This Chapter

The information presented in this chapter is drawn from several sources: from the New Chance Management Information System (MIS), the most detailed source of information on experimentals' activities in the program;[2] from interviews with program coordinators and other key personnel concerning program operations; from responses to questions on the 18-month survey eliciting experimentals' views of the program; and from data collected for the cost analysis.

The rest of this chapter is divided into five sections. After this introductory section, the second section discusses the New Chance program structure and the services offered in Phases I and II. The third section examines data on the extent of participation in these services.[3] The fourth section summarizes the findings of a special study of the quality of program-related child care centers, while the fifth section presents enrollees' opinions of the program and their ratings of its service offerings. The chapter closes by providing new estimates of program costs.

---

[1] Throughout the report, the terms *component*, *service*, and *activity* are used interchangeably to refer to specific kinds of assistance (e.g., education or skills training counseling) called on by the program model.

[2] However, as noted in Chapter 4, the MIS is incomplete as a source of data on *all* of the activities in which experimentals took part. First, it excludes post-New Chance activities and others of which program staff were unaware. Second, time spent in individual counseling was not recorded on the MIS, since counseling sessions were often informal and unscheduled. Third, the MIS did not include hours of college attendance or job placement assistance. Finally, it includes time spent in health education classes, but not receipt of health care services.

B. A Preview of the Findings

In general, the Phase I components of New Chance were put in place as planned, and 88.8 percent of the enrollees participated in the program. Participants' survey responses indicated that they judged the services to be helpful: Enrollees rated all dimensions of program operations about which they were asked higher than 5 (the midpoint) on a 0 to 10 scale. However, service receipt was much lower than expected because of frequent absenteeism – a serious problem in New Chance as in many other programs serving disadvantaged youth (see, e.g., Auspos et al., 1989; U.S. Department of Education, 1988; Higgins, 1988) – and because many of the young women dropped out of the program relatively early. Thus, in practice, Phase I was as comprehensive as planned, but not as intensive. 42.0 percent of all enrollees, and 64.5 percent of those who received a GED, moved on to the employment-focused services constituting Phase II: occupational skills training and paid or unpaid work internships.[4] Most of those who did not take part in Phase II activities had left the program without getting a GED. Some of the New Chance enrollees who had a high school diploma or GED entered college, which, while not part of the program model, was an important goal for many participants.

Other findings indicate that the quality of child care provided by the centers under study was generally good, comparing favorably with the care delivered at a national sample of centers serving low-income families.

To operate New Chance, sponsor agencies spent an average of $5,073 per experimental, excluding child care costs; child care amounted to an additional $2,573 per experimental.[5] These are *gross costs*; the *net* costs of New Chance will be presented in the 1996 report. (Net costs are program costs minus the costs of services received by controls; as previously discussed, controls were free to seek services, but not through New Chance, and many of them did so.)

---

[3] This chapter's analysis of participation goes beyond the data presented in the first implementation report on New Chance (Quint, Fink, and Rowser, 1991) in three principal ways: It supplies information on participation in activities for all experimentals, not just those who entered the program early; it follows them up for a full 18 months after program entry (i.e., after random assignment); and it examines participation in Phase II activities, which usually took place later in a participant's program tenure.

[4] Data on receipt of job placement assistance, another Phase II service, were not collected.

[5] This figure, as discussed below, is not an *annual* cost. It includes costs associated with all enrollees, regardless of the extent of their participation in program activities.

II. The New Chance Structure and Components

A. Program Structure and Service Sequence

New Chance was implemented in a variety of contexts. The 16 New Chance sponsor agencies (program operators) were located in different areas and served different populations. (The agencies are listed in Table 1.1.) The Bronx, Detroit, Harlem, Philadelphia, and Pittsburgh sites all might be characterized as "inner city," drawing enrollees mostly from non-white, high-density poverty areas. Other sites enrolled participants from a much larger geographical area. The Chicago Heights site, e.g., drew its enrollees from a sprawling area of largely working-class and poor suburbs south of Chicago; the Denver program attracted participants from all over the metropolitan Denver area. Appendix C presents salient characteristics of the larger communities in which these sites were located.

As noted in Table 1.1, the sponsor agencies also were different kinds of institutions – community service organizations, schools and a community college, a Private Industry Council (the nonprofit entity that administers funding under the federal Job Training Partnership Act, JTPA), and a county government agency – which, at the demonstration's outset, had a variety of missions. (Thus, while several sites had been concentrating primarily on adult education, the delivery of family planning and health services had been the central focus of the Jacksonville sponsor agency, and the prevention and treatment of child abuse and neglect had been the key mission of the Lexington sponsor.) Although the sponsor agencies selected to run New Chance were all experienced program operators, about one-third of the sites had never before managed a program specifically targeted for young mothers, and those that did largely served in-school youth and non-AFDC recipients. Mounting a new and comprehensive program posed many challenges: Sites simultaneously had to firm up staffing arrangements and components, recruit enrollees, and develop policies on attendance and performance, issues discussed in this chapter.

There was considerable variation by site in how the program services were assembled. All sites had a core staff, which included a New Chance coordinator and one or more case managers, but they adopted different methods of filling the remaining positions. For instance, at some sites, GED instructors were employees of the sponsor agency, while at others, they were provided to the site through an agreement with the local school system, which remained their official employer. Also, as an alternative to directly hiring personnel, the New Chance sites commonly developed linkages with other community agencies (e.g., neighborhood clinics or Planned Parenthood) that could provide experienced instructors in health education, family planning, and parenting. Sites differed, too, in the number of case managers and instructors on board. For example, Pittsburgh's New Chance staff included three full-time case managers in addition to a relatively large number of instructors for the other components. In contrast, Chicago and Chula Vista each employed one case manager, who was also responsible for teaching a component. The number of New Chance full-time equivalent staff positions ranged from three in Denver to 16 in Portland and 17 in Pittsburgh. Different staffing configurations have implications for program costs, as discussed in the last section of the chapter.

Recruiting participants required ongoing effort on the part of program staff. The very newness of New Chance was itself an obstacle to recruitment, especially at the outset, because many sponsor

How One Site Put the Program Model in Place

The Jacksonville New Chance program was operated by Family Health Services (now The Bridge of Northeast Florida), a community-based organization created to provide reproductive health services to poor women and teenagers. In 1982, Family Health Services founded The Bridge, a multi-service center for children and youth, which had prevention of teen pregnancy and related health problems as its original thrust. Over the years, The Bridge's mission has been expanded to address a range of issues affecting the life prospects of inner-city children, teens, and the children of adolescents. For example, in 1986, to augment the parenting skills of teen mothers whose children were enrolled in Family Health Services' primary care program, The Bridge began providing a series of weekly parenting support classes. Broadening its emphasis on prevention services, The Bridge also began offering developmentally oriented recreation activities and activities aimed at strengthening literacy skills to children of both sexes, aged 6 to 18.

New Chance built on The Bridge's expertise in providing health, family planning, and parenting services to the young mothers. However, the agency had no experience in offering employment services, and education services were geared toward homework assistance, tutoring, and augmenting reading skills rather than provision of a full course of basic education and GED preparation. In addition, the Bridge had no history of offering case management. Those parts of the New Chance model that involved employment-related services, full responsibility for providing education services to young women not enrolled in public school, and case management were all new areas for the agency; it therefore had to decide whether to try to acquire expertise in these areas by hiring new staff or to develop arrangements with other agencies that already had the required expertise.

The Bridge decided to link with the local community college – the Florida Community College at Jacksonville (FCCJ) – to provide New Chance's education component. FCCJ, and particularly its nearby downtown campus, also was seen as the major source of skills training courses for participants. The city's JTPA agency, the Private Industry Council (PIC) of Jacksonville, agreed to become a partner in providing New Chance's employment-related activities. The PIC offered the program a way of providing paid work internships for some participants, and took on responsibility for the program's employability development classes and for placing program completers in jobs. Case management, as envisioned in the New Chance model, is very difficult to contract out to another agency; the Bridge therefore hired two case managers for the program.

The consistency and flexibility that FCCJ and the PIC provided in their partnership with The Bridge were advantageous to effective implementation of services. FCCJ allowed The Bridge to interview members of the college's GED staff who were potentially interested in assignment to the New Chance program on a full-time basis; when none had the requisite background in working with teens, The Bridge was allowed to interview candidates not associated with the college and refer preferred candidates to the college for approval. This flexibility allowed The Bridge to hire experienced education specialists who had skills in working with teens and young mothers. The funding arrangement for the educators' salaries has been consistent and is still in effect.

(continued)

How One Site Put the Program Model in Place (continued)

A different kind of long-term arrangement for in-kind support was developed with the PIC. The PIC assigned a staff person with experience in working with youths to New Chance on a part-time basis to conduct the program's employability development classes and develop jobs for participants. Both the staff person assigned to New Chance and the in-kind arrangement for funding the position remained constant throughout the operational phase of the demonstration, which allowed the PIC staff person to become part of the team delivering services, with a mutual exchange of expertise.

One challenge in mounting New Chance could not be addressed through partnerships with other agencies: operating a daily, long-term program for young women that involved an integrated approach across several different service areas and extensive case management. The Bridge's experience had been with shorter-term services for young mothers, some delivered on an appointment or as-needed basis; many new policies and procedures therefore had to be developed and implemented. Here, The Bridge relied on ongoing review and modification of policies, procedures and expectations, debate among the staff team, and consultation with participants to create and refine the procedures and approaches needed to offer a long-term, cohesive program.

agencies lacked a record and a reputation in their communities for running this kind of program. Almost all sites relied heavily on the local welfare agency to identify and conduct outreach to potentially eligible young women (e.g., by including notices about the program in welfare mailings).[6]

Although not required to do so by the program guidelines, all sites except one (Portland) elected to operate New Chance as a program in which GED preparation and vocational skills training took place sequentially rather than concurrently. That is, the program was divided into two distinct phases. The Phase I components -- education (i.e., adult basic education and GED preparation classes), employability development (i.e., career exploration and pre-employment skills training), workshops on life skills, health, family planning and other personal development services, and parenting classes -- were generally delivered on-site; in contrast, Phase II activities -- occupational skills training and work internships -- were usually off-site.

Several factors induced most sites to arrange education and training sequentially: the desire to incorporate parenting and personal development along with education into the first phase of the program, leaving less time for other services; the belief that the young women needed stronger educational preparation to succeed in skills training; and the fact that many skills training programs would not accept applicants who did not have a high school diploma or GED in hand, or would not allow them to complete or advance within the courses.

---

[6] Many local program coordinators believed that they had had to accept relatively unmotivated young women, or those with multiple problems, into the program in order to meet random assignment quotas. At the conclusion of random assignment, several sites instituted more selective screening procedures.

The Portland site established a different program structure. The New Chance program in Portland was operated jointly by the Portland Public Schools and the Job Corps and adopted a program model in which skills training took place concurrently with other program activities. Thus, in Portland there was no real distinction between Phases I and II.[7]

The guidelines set a maximum length of stay of 18 months in the program.[8] The guidelines further stipulated that participants should move on to skills training or a work internship after five months, to reinforce employment as the program's ultimate goal and to curtail boredom and discouragement; and they stated that skills training should generally be no shorter than three months and no longer than 12 months. Together, these requirements reflected the expectation that it would take young women the better part of a year or longer to attain first a GED and then a certificate signifying the successful completion of skills training.

B. Implementation of Phase I Activities

A detailed analysis of early program operations may be found in Quint, Fink, and Rowser (1991), where the issues involved in establishing the infrastructure, recruiting participants, and providing Phase I services receive extended discussion. A major finding of that report was that the 16 demonstration sites put in place all the early program components and with only a few exceptions offered the required hours of each service prescribed by the program guidelines (see Table 3.1).[9] This fact is especially striking given the different service traditions of the sponsor agencies (see Table 1.1).[10] Moreover, whatever the initial orientation or philosophy of these agencies, some services (e.g., education and parenting) generally proved much easier to implement than others (e.g., employability development and family planning counseling).

---

[7] Since the period under study in this report, the Harlem, Minneapolis, and Philadelphia sites have all moved toward a model offering skills training concurrently with education and other activities.

[8] For these purposes, the program was defined as including skills training and work internships but not college, which was regarded as a post-program activity, as was more advanced skills training.

[9] At most sites, life skills, parenting, family planning, health, and employability development were covered in separate classes. However, a Denver instructor combined and integrated these subjects in all-morning classes. In Pittsburgh, participants did not receive the majority of employability hours until they had passed the GED test. In fact, no site's schedule exactly matched the participation requirements in the guidelines because of pragmatic concerns (space or staff availability), convenience, or the belief that a component warranted more (or less) attention than the guidelines command.

[10] Sites did differ, however, in the extent to which MDRC staff viewed them as giving greater emphasis to one or the other of the twin objectives of New Chance: the young women's preparation for employment and self-sufficiency, or their personal development and acquisition of parenting skills. While half the sites were judged to have emphasized both goals equally, four (Allentown, Chula Vista, Minneapolis, and Salem) were deemed to have placed greater emphasis on parenting and personal development goals. Interestingly, at two of these sites (Allentown and Salem), improving parenting skills was a principal mission of the teen parent programs operated by the sponsor agency prior to New Chance; the other two sites had human capital development as their chief aim prior to New Chance. Four sites (Harlem, Jacksonville, Philadelphia, and Pittsburgh) were judged to have given greater emphasis to self-sufficiency objectives; at three of these sites, self-sufficiency was also a strong focus of the sponsor agency before New Chance, but the fourth (Jacksonville) was mainly geared toward family planning and reproductive health issues.

TABLE 3.1

SCHEDULE OF PHASE I NEW CHANCE COMPONENTS, BY SITE

	Days of Program Operation per Week	Scheduled Hours per Week					Scheduled Group Sessions per Month	Scheduled Individual Meetings per Month
				Health and Personal Development				
Site		Education	Employability Development	Life Skills	Health Ed.	Parenting Education	Family Planning	Case Management (a)
Allentown	4	10	2	1.5	2	1.5	2	2
Bronx	5	15–20 (b)	3	2.5	2	3–4 (b)	1–4 (b)	1
Chicago Heights	4	12	3	1.5	3	3	2	2
Chula Vista	5	20	2	3	2	2	2	1
Denver	5	10	4	1.5	4	9	4	3
Detroit	4	12	2	1–2 (b)	2	1.5	1	1
Harlem	5	15	1.5	1.5	1.5	1.5	4	1 (c)
Inglewood	5 (d)	10	8	3	1.5	1.5	1	1–2
Jacksonville	5 (d)	14.5	5	1.5	1	2	1	2
Lexington	4	13	2	2	2	3	1	2
Minneapolis	5	12.5	2.5	2.5	2.5	2	-- (e)	2–3
Philadelphia	5	13.5	1.5	2	2	2	2	1
Pittsburgh	5	6.5–19 (f)	2–9 (f)	1.5	2	1.5	1	4
Portland	5 (d)	10–15 (g)	5	3	1	2	1	2
Salem	4	12	3.5	3	1.5	3	1	-- (h)
San Jose	5 (d)	13	3	1.5	2	3	2	-- (h)
Guideline requirements	4–5	12–15	48 hours before work internship or skills training	18 90–minute sessions	1.5	2–4	1	2

(continued)

TABLE 3.1 (continued)

SOURCES: New Chance program records and staff interviews.

NOTES: Several services provided by New Chance programs are not included in this table: adult survival skills training, which at some sites was scheduled as a separate class and at others was incorporated into other components; occupational skills training, which was offered during Phase II of the program (except in Portland and Harlem, where participants attended occupational skills training concurrently with the education, health and personal development, and employability development components); and other group activities (e.g., group meetings or field trips).

(a) Case management was provided in Phase II as well.

(b) The number of component hours varied, depending on the weekly program schedule.

(c) At this site, case managers originally scheduled three to four meetings per month, but as the number of participants increased, case managers reduced the number of scheduled meetings.

(d) On Fridays, only education classes were scheduled.

(e) The site operated on a module system, devoting a one-week module to family planning.

(f) Participants were placed in one of four class levels depending on their educational ability: pre-GED 1, pre-GED 2, GED, or career orientation. The number of hours a participant spent in education and employability development activities varied depending on the class level.

(g) Depending on their Tests of Adult Basic Education (TABE) results, participants were required to attend either two or three hours of education classes per day.

(h) Case managers met with participants informally, but no formal case management meetings were scheduled.

This section describes the Phase I components. (Child care is discussed in a later section of the chapter.) It should be stressed, however, that New Chance is more than an assemblage of services. Central to it are the *relationships* enrollees form with staff members and with one another. To promote a sense of closeness and separate identity, New Chance enrollees attended most activities separately from other clients of the sponsor agency. A separate physical location – a series of rooms or a separate building – also fostered the development of a distinct group identity and spirit.

1. **Enrollment and Orientation.** Some sites opted for "cohort enrollment," whereby groups of participants started program activities only at certain designated times (e.g., once a month or twice a year). Others chose an "open enrollment" policy, which allowed young women to join program activities as soon as they had been randomly assigned to the experimental group. There were advantages and drawbacks to each procedure. Open enrollment allowed young women to start right away, but placed a burden on case managers and other staff to make new arrivals feel welcome.[11] Cohort enrollment fostered a feeling of camaraderie among young women who entered the program and underwent common experiences together. Sites could not always recruit a sufficient number of recruits to form a new cohort, however. A further difficulty of cohort enrollment was that during the delays that sometimes occurred between random assignment and the start date of the next group – delays that could last several weeks – some enrollees found other programs that appealed to them more or otherwise lost interest in New Chance or underwent changes in their situations (e.g., pregnancy or homelessness) that made their participation impossible.[12] Despite these problems, which at some sites were substantial, the New Chance program coordinators, when interviewed at the end of the demonstration period, expressed a preference for cohort enrollment because of the strong group cohesion it engendered.

During the orientation period, whether formal (conducted in groups and often of several days' duration) or informal (conducted individually, and usually much briefer), staff members stressed to new participants the benefits of being in New Chance and reiterated their own willingness to support the young women at every juncture. Staff also explained the rules and regulations that participants were expected to follow. Some sites were initially unwilling to "hit participants with rules at the beginning," but over time, in response to absenteeism problems, they placed increased emphasis on the importance of daily attendance.

Assessment commonly began during orientation as well. Case managers met individually with participants and tried to detect and resolve situations, such as lack of transportation, that could undermine a participant's efforts to attend the program. Tests to determine educational needs and occupational interests and capabilities also allowed staff to get to know participants. Finally, social and recreational events were another vehicle for introducing new participants to the program and to one another.

---

[11] Some open enrollment sites instituted a "buddy" system, whereby a newcomer was paired with a young woman who had been in the program for some time.

[12] Because they had been randomly assigned to the experimental group and formally enrolled in the program, however, they remained part of the study and were followed up in the 18-month interviews no differently than experimentals who actually participated in the program.

2. **Education**. The New Chance guidelines state that the education component must include reading, writing, and basic mathematics (which collectively constitute adult basic education, ABE); an introduction to and preparation for the GED test, which covers a number of academic subjects; and an introduction to computers and their applications (in sites where computers were available). The widespread availability of GED curricula and of teachers experienced in teaching GED or adult basic education classes, and the fact that most sites were already running education classes, made it relatively easy to operate an education component. The challenge, especially at sites that relied on linkages with other agencies for this component, was to find instructors who were not only knowledgeable about the subject matter but also attuned to the students' needs and abilities, and to the program as a whole.

Participants' own interest in getting a GED also contributed to the relative ease with which the component was implemented. Indeed, the opportunity to earn this certificate was the major reason many enrollees gave for joining New Chance, and they spent more time in education than in any other program service. Most sites separated participants into different classes according to their level of academic skills at entry. Staff reported that finding adequate instructional materials and motivating participants with low reading or math skills was difficult: The young women needed constant reinforcement and reassurance that they were improving, and too great an emphasis on the GED could be discouraging to those unlikely to pass the test for some time.

Education teachers espoused different preferences and followed different practices for group or individualized instruction. Individualization at most sites meant that students were assessed for their skills levels in math and reading and given appropriate materials in each subject; they then spent the majority of their time working on their own, with a workbook or text suited to their academic level and with instructors available to answer questions and work one on one with students. Philadelphia was the only site that relied primarily on group work in the education classes; several sites used a combination of group and individual work.

Over time, computer-assisted instruction was used to supplement more traditional education at most sites. However, computers became an effective mode of instruction only after sites acquired an adequate number of them and found the appropriate software, and after instructors became more familiar and comfortable with both the software and with computers in general.[13] Participants at some sites attended a special computer lab once a week; at other sites, students spent up to half their regular class time working on computers. Staff noted that participants enjoyed working on computers because the machine permitted feedback that was both immediate and private, protecting the young women from feeling embarrassed in front of their peers.

3. **Parenting Education**. The goal of parenting classes was to help participants become better parents by enhancing their ability to foster their children's cognitive, social, emotional, and physical development. Parenting, like education classes, was a fairly easy component to implement, even for sites that did not start with a focus on it, partly because parenting instructors had fewer New Chance responsibilities than did other staff and could therefore devote somewhat more time and

---

[13] Apple Computer, Inc., made an in-kind contribution of computer equipment to five sites to enhance their computer-assisted instruction capabilities.

attention to their subject. In addition, MDRC trained parenting instructors with a curriculum designed for use with disadvantaged young mothers.

The guidelines for parenting classes discouraged lectures and encouraged active, participatory sessions. The guidelines also set forth various topics to be covered, such as child development, coping with stress, and locating appropriate child care providers. A major focus of the component was to teach participants about the developmental stages of childhood so that they could better recognize and understand age-appropriate behavior, learn how to stimulate their children's mental and physical development at different stages, and use appropriate discipline. In addition, at least once a month, sites were to offer hands-on, interactive parenting sessions in which both mothers and children were present. These sessions could not always be easily implemented (especially when sites lacked on-site child care), but when they were, they allowed participants to have fun with their children, while giving parenting instructors an opportunity to diagnose problem areas and model positive parenting behavior.[14]

Staff reported that participants were interested in learning how to accomplish specific goals (e.g., toilet training). They also sought reassurance that they were not alone in their parenting problems. Many participants, however, did not recognize a need for parenting classes, seeing themselves as good mothers already.

4. **Employability Development.** Employability development combined career exploration with pre-employment skills training. The focus of career exploration was on acquainting participants with various fields and careers; helping them understand the duties, education prerequisites, and skills requirements of a range of jobs in which they expressed interest; and exposing them to people actually doing those jobs. Several instructors took participants to job fairs featuring employers in different lines of work, and most sites brought in occasional guest lectures. The pre-employment skills class covered job search techniques, applications, resumes, appropriate dress for the workplace, employer expectations, and job-keeping strategies. A vocational assessment, usually completed shortly after a young woman enrolled in the program, was a tool used to help staff and participant prepare an individualized plan detailing the steps between program entry and a job.

As noted above, employability development was difficult to implement well, for many reasons. Often the person responsible for teaching it was also responsible for placing participants in skills training or work internships. (In other instances, case managers were responsible for skills training and work internship placements along with their other responsibilities.) Thus, the position of employability development instructor required a wide range of skills and capabilities, and sites found it hard to find individuals who could do all these things well. Over time, some sites did find instructors able to implement the more difficult facets of the component (e.g., career exploration) and to generate enthusiasm among participants for the employment side of the program, but this was not an easy task. Smooth functioning of the component was also hindered by sponsor agencies' inexperience with this service and by the absence of a suitable curriculum.

---

[14] At some sites, child care staff also helped parenting instructors learn about the young mothers' strengths and weaknesses as parents. This was especially the case at Allentown, where staff of the on-site child care center recorded their observations of the participants' interactions with their children daily and reviewed their notes with other staff members at weekly meetings.

Finally, staff reported that many participants were not very motivated to enter skills training or jobs. Their primary objective in the program was to pass the GED test, and they did not plan far beyond that immediate goal. Furthermore, staff described program entrants as unrealistic in their expectations – knowing little about the skills various occupations require, or expecting to be able to get a good job with just a GED in hand. Changing attitudes such as these was a challenging task.

5. **Family Planning.** An important objective of New Chance was to provide participants with the knowledge and services needed to postpone further childbearing, and to assist them in developing the motivation to do so, until they were in a better position to provide for their families. Staff were generally in strong agreement with this objective. The program's family planning component included three aspects: education classes or workshops, individual counseling, and linkages with family planning service providers. The guidelines specified that classroom instruction should be provided by trained staff at least once a month (with two additional classes to be scheduled during orientation), that approved curricula should be used, and that case managers must counsel each participant regularly on her family planning practices.

The requisite monthly classes on family planning generally proceeded as planned. The existence of public and private agencies specializing in health and family planning made linkages with such organizations an appealing option for obtaining trained staff for the service. Linkages with family planning providers were strongest where clinics were located on the program site or at another branch of the sponsor agency, but some sites without on-site clinics were able to develop effective linkages with off-site providers for both instruction and services.

A major issue for the program as a whole, however, was that at a number of sites, case managers did not routinely or effectively counsel participants about their use of contraceptives. Some case managers resisted this role because they were uncomfortable dealing with the subject of sexuality. Others felt that they lacked the required expertise about family planning methods. Still others were comfortable with the subject but, given the limited time they had to spend with each participant, tended not to discuss family planning unless the young woman raised it as a specific problem. Changes in participants' lives that might lead to failure to continue with a contraceptive method tended to go unaddressed.

The family planning part of the program posed difficult challenges because staff not only had to provide participants with knowledge about contraceptive methods, but they also had to instill within them the motivation to use these methods regularly. Staff members noted that changing participants' behavior was complicated by the presence of new partners in their lives, and by the fact that many participants came from communities where women did not traditionally assert themselves.

6. **Health Education and Services.**[15] Participants' health-related attitudes, behavior, and practices affected not only their own health but also that of their children. The health component sought to improve participants' health habits and help them develop healthier lifestyles by emphasizing the importance of preventive care and by facilitating their access to health care services. Health education was provided on-site, while health care services were available through hospitals and clinics and, at three sites, at on-site health care facilities.

---

[15] This discussion of health care services includes both maternal and pediatric care.

As prescribed by the guidelines, the health education segment included a wide range of topics: AIDS, use of community resources, children's illnesses and immunizations, physical and emotional abuse, women's reproductive health issues, and the role of preventive care. Some sites also discussed depression and stress management and offered classes on cardiopulmonary resuscitation (CPR). In addition, information about drug and alcohol abuse was conveyed. At some sites, staff members reported that the participants themselves had substance abuse problems; at others, they indicated that substance abuse was a common problem among participants' family members or partners.[16]

Health classes were commonly integrated with parenting and family planning classes. For instance, AIDS and other sexually transmitted diseases were often discussed in both family planning and health classes. This integration was facilitated by the fact that the majority of health instructors also taught family planning. Similarly, such issues as household safety and when to take a child to a doctor were related to parenting as well as to health.

The guidelines also required that participants receive a complete health examination soon after enrollment and that free health care (including vision and hearing examinations and corrective services, along with dental care) be available for participants and their children, through on-site clinics, linkages with specific hospitals or clinics, or the medical facilities participants were already using. The sites with on-site clinics were more likely to operate in conformity with these guidelines. Elsewhere, a few sites developed linkages with health care providers and made an effort to monitor receipt of services by enrollees and their children, but many did not, partly because participants often used clinics near their homes and were therefore spread out among numerous providers.

7. Life Skills. Life skills in New Chance comprised two elements. Life Skills and Opportunities (LSO), a separate class built around a curriculum especially developed for the demonstration, was designed to foster skills in decision-making, effective communication, assertiveness, problem-solving, contingency planning, and working in groups. It addressed these skills in the context of four main areas of participants' lives: sexuality, relationships, parenting, and the world of work. Although these areas were also covered in other components, the objectives of LSO differentiated it from the other classes. Staff members agreed that participants' favorite topics included sexuality, male-female relationships, and assertiveness; they were less responsive to the sessions on breaking stereotypes and combining work and family.

LSO was also distinguished by the structure and format of the classes, which concentrated on participants' involvement in structured activities and discussions of their ideas, feelings, and beliefs. Each session consisted of a variety of activities, such as a group discussion and role-playing, all addressing the same topic.

Several instructors felt that the curriculum was useful, but that, given the tendency of teenagers to "live for the moment," participants were unlikely to change their decision-making habits immediately. These instructors hoped, rather, that the component would teach participants skills they might put into practice at a later point, even if they did not immediately appreciate their relevance.

---

[16] Portland arranged for counselors from another community-based agency to provide drug and alcohol education, individual assessments, and, if necessary, referral for treatment.

The second life skills element, known as adult survival skills, was not a separate class but consisted of a variety of practical skills that were to be included in the curricula of other components. (However, one staff member was responsible for seeing that the other components incorporated these skills.) The guidelines defined adult survival skills as consisting of the following areas and topics: money management (e.g., budgeting, banking, taxes, and credit); transportation (e.g., reading maps and schedules); time management (e.g., using alarm clocks, making schedules, setting priorities, and establishing contingency plans); getting information and personal records (e.g., using public agencies, libraries, and newspapers); rights and negotiations (e.g., knowing about anti-discrimination laws, child support, voter registration, and Unemployment Insurance); and interpersonal skills (e.g., assertiveness, effective use of the telephone, and conflict resolution).

8. Case Management. New Chance case managers had multiple responsibilities, including orienting new enrollees to the program, assessing participants, monitoring progress, counseling individuals, keeping records, and advocating on the young women's behalf. In addition, three-fourths of all case managers also taught one or more classes in New Chance and/or were responsible for recruiting new enrollees.

The ongoing relationship between the case manager and the participant demanded sensitivity, individualized attention, and guidance. For this reason, in hiring case managers, program coordinators sought such qualities as good communication skills, counseling experience, teaching credentials, and enjoyment in working with young people. Coordinators also looked for case managers who could serve as role models for participants.[17]

Case managers with teaching responsibilities could directly monitor participants' behavior and attitudes in their class. "Case conferences," at which all staff discussed certain participants, helped case managers monitor participants' progress in other components as well. When a participant was absent, the case manager or another staff member called her, usually that day or the day after.

At more than half the sites, case managers did not adhere to the guidelines' requirement that meetings with participants be scheduled every two weeks. Especially as their caseloads increased, many felt that they had too many other responsibilities to adhere to a formal schedule of meetings. In addition, some case managers felt that participants were resistant to scheduled meetings and more open during informal conversations. Case managers at all sites met with participants more often on an informal than on a formal basis, and participants often visited their case managers during lunch hour or breaks to discuss problems.

At virtually all sites (except those with low recruitment or poor retention rates), caseloads at some point exceeded the maximum recommended size (i.e., caseloads no larger than 25, and no larger than 15 if case managers had other responsibilities). At most sites, one or two staff members were assigned as case managers; as enrollment increased, caseloads often rose into the 40s and, at a few sites, to as high as 60 or 70.[18] High caseloads, coupled with other program responsibilities, forced many case managers to rely on informal meetings with participants, to delay contacting absent participants, and to reduce other monitoring and record-keeping activities.

---

[17] Three sites had male as well as female case managers. One male case manager spoke for his counterparts at the other sites when he expressed his belief that a male case manager allowed participants to receive support and affection from a man without having strings attached.

9. Other Activities. Over time (and sometimes after the period of operations covered in this report), sites expanded their service offerings beyond those mandated in the guidelines to respond to other needs of participants. Pittsburgh, for example, included driver education classes, and others added exercise classes. In a few cases, staff wanted to increase the attention paid to a particular topic within a component (e.g., drug and alcohol abuse) and developed a separate class on that issue.[19] Topics within adult survival skills were sometimes given more prominence by being covered in separate classes. For instance, a few sites provided separate sessions covering nutrition, time management, budgeting, and other practical life skills. Some sites scheduled group meetings on a regular basis to resolve problems among participants, to discuss program rules, or to plan an event; at other sites, these sessions were held only on an as-needed basis.[20]

C. Implementation of Phase II Activities

The activities that make up the second phase of the New Chance program model – occupational skills training, work internships, and job placement assistance – are of particular importance because these are the components that most directly prepare young women for employment. Phase II components differed from the Phase I components in several respects. First, whereas all enrollees in Phase I received essentially the same service "package" (except for case management, which was one-on-one), Phase II activities were individualized to suit the needs and skills of each participant. Second, whereas at most sites staff employed by the sponsor agency delivered most of the Phase I services, setting up Phase II placements and monitoring the young women's performance in them required considerable interaction with outside agencies. Finally, while Phase I activities tended to be implemented to a relatively uniform degree from site to site, there was considerable variation in the extent to which sites offered the Phase II services. (Data on participation in Phase II activities are presented in the following section.)

The transition to Phase II was often difficult for both staff members and participants to negotiate, although some staff members commented that it had grown easier over time.[21] One reason may be that while program staff members gave considerable emphasis to putting the Phase I components in place, at many sites they appeared to give less attention to the Phase II components until participants were almost ready to enter them. Data collected for the cost analysis indicate that, during a period before the majority of program enrollees would have been ready to enter Phase II, sites generally spent little on activities that might be described as "planning ahead": finding work internships or skills training slots, helping enrollees get into college, or counseling participants about the next steps.[22]

---

[18] At any time, some percentage of the caseload was inactive. Nevertheless, case managers often spent time working with these individuals, keeping in touch with them and encouraging them to return to the program.

[19] In "wrap-up" interviews conducted with program coordinators at the conclusion of the operational phase of the demonstration, other program coordinators noted the need for a separate component dealing with substance abuse.

[20] A couple of sites developed mentoring programs that matched enrollees with adult women in the community who could serve as occupational and social role models.

[21] The only staff members who reported that the transition was *not* difficult were at sites offering on-site training.

Responsibility for arranging placements of New Chance enrollees in skills training programs, work internships, and employment and monitoring their participation in these activities was vested in a job developer or in the case managers. Shortly after enrollment, as noted above, participants usually took one or more tests designed to measure their vocational aptitudes and interests. The results helped to shape decisions about Phase II choices, as did other considerations: the young woman's preferences, her performance in Phase I, and the availability of various Phase II options.[23]

Once enrollees were engaged in Phase II activities, New Chance staff were responsible for monitoring their attendance (except for those in college) and their satisfaction with these activities. Although program guidelines called for biweekly contacts with Phase II participants, such contacts tended to occur less frequently than that, especially if the young women moved into off-site activities. As caseloads rose, case managers were often occupied with addressing the pressing issues of the new group of enrollees in Phase I, whom they saw daily, and had little time left over for systematic check-ups on young women they did not see. (In contrast, young women who were engaged in skills training or work internships provided on-site by the New Chance sponsor agency were likely to have had much more frequent interaction with their case managers and other program staff.) New Chance staff were also responsible for contacting training program staff and work internship employers to assess enrollees' progress in these components, an activity that was time-consuming and sometimes difficult.

At a number of sites, participants in Phase II activities sometimes also attended parenting classes or social activities at the New Chance site. They also received the support services – e.g., child care and transportation assistance – accorded to Phase I enrollees.

Descriptions of the specific Phase II components follow.

1. **Occupational Skills Training.** Half of the New Chance sites offered on-site skills training.[24] However, only in Portland was this the sole training resource utilized, and the majority of young women who engaged in skills training at the other locations went off-site to do so. There were advantages and disadvantages to on-site training. On the plus side, as noted above, participants who got their training on-site were able to remain in an environment in which they already felt comfortable and to see their case managers more frequently, making it easier for staff to identify and

---

[22] Inglewood, Lexington, Minneapolis, Pittsburgh, San Jose, and, to a lesser degree, Portland all incurred higher-than-average Phase II coordination costs. The data presented in Table 3.5, however, do not suggest that higher expenditures were necessarily related to a higher proportion of enrollees entering Phase II activities.

[23] Availability was a matter of timeliness as well as openings. Thus, some sites sought to place participants in work internships for a few weeks or months until a new skills training cycle or college semester began.

[24] These eight sites were: Allentown, the Bronx, Chula Vista, Denver, Harlem, Inglewood (for only a brief period during the demonstration), Portland, and San Jose.

resolve problems early on and perhaps facilitating participation. On the minus side, however, most sites offered training only in limited areas. (For example, the Portland site – whose implementation of Phase II was notably strong – provided training only in business skills.)

In general, participants were interested in a relatively narrow range of "pink-collar" occupations, and typically they sought training in clerical areas (as data processors, business machine operators, and the like) and in medical fields (as medical technicians or, frequently, as certified nurse's aides).[25] The decision about which training provider would be most suitable rested on several factors: location; entry requirements (some facilities required enrollees to have a GED upon entry, whereas others allowed them to work toward a GED concurrently with their training); when slots were available; whether training was free to the participant (New Chance staff counseled participants not to enroll in programs that required them to take out loans); and the facility's general reputation and placement record (as well as its record with previous enrollees from New Chance). Training was typically financed under the JOBS program, by funds under the Job Training Partnership Act (JTPA), or (when offered through community colleges) by Pell Grants.[26]

New Chance sites differed in the degree of difficulty they experienced in making appropriate training placements. Chicago Heights staff, for instance, found not only that it was hard to obtain slots for young women who did not have a GED, but also that the majority of programs were in locations that were hard for program participants to get to. The Detroit program, in contrast, was able to exploit the connections and contacts of members of the program's Advisory Board to find suitable placements for participants.

2. **Work Internships**. Work internships were designed to provide participants with exposure to the practices and routines of work settings in general, as well as to the tasks and working conditions associated with specific kinds of jobs. Sites differed in the extent to which they made use of work internships and in their judgments as to which young women were appropriate for this service. Detroit, e.g., tended to use work internships for participants with relatively low skills, while Minneapolis staff placed in these positions only young women with a record of good attendance in Phase I, reasoning that otherwise the agency's reputation with employers would be compromised.

Work internships varied in intensity and duration. At one site, e.g., they were scheduled for four hours a week for six months, while at another, they occupied 15 to 20 hours a week and lasted between two weeks and three or four months. At some sites, participants worked for the experience alone; at others, they were paid (in Denver, e.g., the pay was between $4.50 and $6.00 an hour).[27]

Some sites relied heavily on already existing pools for work internship positions. In Minneapolis, many New Chance participants were placed through a work experience program for the disadvantaged operated by a city agency. At several sites, New Chance participants got temporary jobs through the federal Summer Youth Employment Program; at all the sites that drew on pre-existing pools, however, New Chance staff also developed individualized work internship positions for young women with specialized interests or needs.

---

[25] As noted in the 1991 implementation report, while the New Chance sites did not make a concerted effort to interest the young women in nontraditional occupations, it is also true that only a handful of young women expressed interest in such careers.

[26] Pell Grants are federal grants-in-aid given to economically disadvantaged college students.

[27] Where internships were paid, an agreement was usually reached with the welfare agency whereby the stipends were not deducted from the young woman's welfare check. However, at one site, participants could not accept paid internships because they would then have been ineligible to receive child care.

Program staff reported that, in general, young women enjoyed the work internships as long as they were doing work that was meaningful and challenging, but also that they were apt to feel exploited if their work was too similar to that of regular employees who were being paid much more.

3. **Job Placement Assistance.** Responsibility for job placement assistance was lodged in different parties at the different sites. At some sites (e.g., Denver), this was primarily the province of the off-site training programs to which the young women were referred. (Those who took part in Denver's on-site training were required to attend a job search component operated by the sponsor agency.) At other sites, New Chance or other sponsor agency staff took on this function; the program coordinator at the Minneapolis program, e.g., reasoned that the sponsor agency's good relationships with local employers would greatly facilitate placements. At still other sites (e.g., Chicago Heights and Detroit), participants were responsible for developing their own job leads, with the assistance and encouragement of program staff. Finally, at a number of sites, responsibility for placement depended on the status of the young woman to be placed. At Pittsburgh, e.g., the job developer worked principally with GED recipients, both those who had completed training and those who had not.

4. **College.** For some young women, enrolling in college was a dream that New Chance helped make a reality. The New Chance sites differed in their policies about which young women should be encouraged to attend college. Staff at some programs, reasoning that college was difficult and demanding, believed that this option should be reserved for young women who had displayed regular attendance and greater-than-average maturity. Minneapolis was concerned that prospective college students' close relationships and housing arrangements be stable. Other sites felt that all students who wanted to attend college could benefit from this experience, and that a college diploma would better enable the young mothers to attain long-term economic self-sufficiency.[28]

While not part of the formal program model, college attendance was a post-GED activity for some of the young women; those who attended college generally enrolled in two-year community colleges, where they took a mix of liberal arts (e.g., English and psychology) and vocationally oriented courses. According to program coordinators, however, the young women often discovered that completing a GED was inadequate preparation for college: They were unaccustomed to lectures and note-taking, to demanding schedules, and, especially, to lengthy reading assignments. Dropping courses was common. Progress toward a diploma was also impeded by the fact that many young women were required to take remedial-level courses in English and math.

---

[28] Some sites changed their assessments over time. The Lexington program coordinator, e.g., initially believed that a college degree would give young women an important advantage in the area's generally low-wage economy. The high rate at which New Chance students dropped out of the local community college caused her to reassess this position and to see training programs as a more suitable option for many enrollees.

III. Participation in New Chance Activities

This section presents data on participation in Phase I and Phase II activities, first for the experimental sample as a whole and then for subgroups of the sample and for the individual sites. It then discusses the reasons for absenteeism and the ways in which the local programs tried to address this issue.

A. Aggregate Measures of Participation

Table 3.2 shows several measures of participation for all New Chance enrollees (i.e., all experimentals), following the young women's activities for 18 months after random assignment.

The table makes several points. First, as expected, a high proportion – 88.8 percent – of the young women participated in some program activity.[29] Participation was less than universal because, as noted earlier, there was attrition during the sometimes lengthy waiting periods between random assignment and the actual start of program activities.[30]

Second, a large majority of the young women received each of the Phase I services. Almost 86 percent attended education classes, and between 70 and 80 percent of the young women took part in employability development, family planning, health education, parenting instruction, and life skills workshops. Given the high number of early terminations from the program, however, it is not surprising that much smaller proportions of the group took part in Phase II activities. About a third (33.0 percent) participated in skills training, 21.0 percent in work internships, and (not shown in the table) 12.5 percent in college.

Third, on average, the young women participated for just under 300 (297.6) hours in counted activities. About a third of these hours were spent in education. Although only a third of the young women participated in skills training, those who did so were in this activity for several hours a day, bringing the overall average to 67 hours. Health education and personal development activities (e.g.,

---

[29] A young woman was considered to have participated in New Chance if she attended one of the activities shown in the table for at least one hour. Individual counseling sessions were not recorded on the MIS. It is likely that some young women who did not take part in other program activities received some counseling; thus, a "true" measure of program participation would be somewhat higher than the 88.8 percent figure shown in the table.

A comparison of the characteristics of those who ever participated and those who did not indicates that the groups were similar in most respects. However, where there was a statistically significant difference, it tended to suggest that the nonparticipants were more disadvantaged: They had lower educational aspirations and had been out of school longer than the participants. As a group, nonparticipants were younger; they were also less likely to have fathers who were employed, to be using birth control, or to have a child support order, but more likely to have married.

[30] A question on the 18-month survey asked young women why they had never attended New Chance. Lack of child care (or lack of child care early enough on), pregnancy, and having moved were the three reasons most frequently cited, and together accounted for about 37 percent of the reasons given for nonparticipation. Other factors mentioned with some frequency were transportation difficulties, family problems, preference for another program, and unwillingness to attend the classes.

TABLE 3.2

PARTICIPATION RATES, HOURS OF PARTICIPATION, AND MONTHS OF ACTIVITY FOR NEW CHANCE EXPERIMENTALS WITHIN 18 MONTHS AFTER RANDOM ASSIGNMENT

Activity Measure	Experimentals
Participated in (%)	
Any activity (a)	88.8
Education	85.7
Skills training	33.0
Work internship	21.0
Employability development	79.2
Family planning	72.2
Health education	72.2
Parenting education	79.2
Life skills	80.0
Other group activities	79.2
Average hours of participation in	
All counted activities (a)	297.6
Education	101.0
Skills training	67.4
Work internship	28.4
Employability development	26.5
Family planning	6.3
Health education	10.5
Parenting education	17.6
Life skills	20.5
Other group activities	19.3
Percentage distribution of hours in all activities	
0	11.2
1-100	24.9
101-300	25.1
301-500	16.5
501 or more	22.3
Total	100.0
Months of activity (b)	
Average	6.4
Median	5.0
Still participating in the specified month after random assignment (c) (%)	
Month 3	75.4
Month 6	59.4
Month 9	44.2
Month 12	32.7
Month 15	22.2
Month 18	11.6
Sample size	1,408

(continued)

TABLE 3.2 (continued)

SOURCE: MDRC calculations from New Chance MIS data.

NOTES: Calculations for this table used data for 1,408 experimentals for whom there were 18 months of follow-up survey data, including values of zero for those who were randomly assigned to New Chance but did not participate. The table includes 18 months of MIS follow-up data for each individual.

(a) Excludes individual counseling and college classes.

(b) Number of months in which experimentals took part in New Chance activities may not have been continuous.

(c) Includes women who had dropped out but subsequently rejoined the program.

parenting education and life skills instruction) generally occupied between 10 and 20 hours each.[31]

Fourth, this 297.6 average is just that: an average. There was, in fact, wide variation in the distribution of hours of program activities attended. Along with the 11.2 percent of all experimentals who did not participate at all, another 24.9 percent participated for 100 hours or fewer. At the other end of the spectrum, 22.3 percent registered more than 500 hours.

Fifth, on average, experimentals were active in the program for 6.4 months. These months were not necessarily continuous, however, so that "months of activity" is not synonymous with "length of stay." Periods of program activity were sometimes interspersed with periods in an "inactive" status; thus, if a woman was active for three months, inactive for two months, and then active for another three months, she would be considered to have been active for six months (but as having had a length of stay of eight months).[32]

About a third of the young women were participating in the program a year after they had been randomly assigned, and just over a tenth were still active at the time of the 18-month follow-up interview. (These figures include women who had dropped out but subsequently rejoined the program.)

These data suggest that, for a majority of enrollees, New Chance was a comprehensive treatment, in that it touched on most areas of their lives and on their roles as students, prospective workers, parents, daughters, and partners. But the statistics also suggest that, in many cases, because of absenteeism and early departure from the program, New Chance did little *more* than touch on these domains; for a sizeable number of enrollees, it was not the intensive treatment that program planners had intended. Data presented in the 1991 New Chance implementation report indicate that, because of absenteeism and early terminations, participants generally got between 30 and 40 percent of the Phase I service "dosage" they could have received. And MDRC operations staff noted that at many sites only about half the young women enrolled in the program attended on any given day.

B. Subgroup Variation in Participation

The aggregate figures presented in the preceding section conceal a good deal of variation both among young women with different baseline characteristics and among the program sites. Table 3.3 shows the average hours of participation for subgroups of young women defined by their characteristics at random assignment. The presence of one or more asterisks indicates that the difference in average participation hours for the different subgroups was *statistically significant* – i.e., unlikely to have arisen simply by chance. (The concept of statistical significance is explained in detail in Chapter 2.) The p-value shows the specific probability that the difference was the result of chance; probabilities of 0.1 or less are considered to be indicative of statistical significance. Thus, on average, young

---

[31] Eighteen months of MIS follow-up were available for all New Chance enrollees. But more than 18 months were available for earlier entrants, and all available follow-up data were used in the cost analysis, as discussed below. Had these extra months of data been included in the average hours of participation shown in Table 3.2, they would have added more hours to the average hours shown for education and skills training.

[32] Sites varied in their use of inactive status and in how quickly they terminated nonparticipants from the enrollee roster.

TABLE 3.3

AVERAGE HOURS OF PARTICIPATION OF NEW CHANCE EXPERIMENTALS WITHIN 18 MONTHS AFTER RANDOM ASSIGNMENT, BY CHARACTERISTICS AT RANDOM ASSIGNMENT

Characteristic and Subgroup at Random Assignment	Sample Size	Average Hours of Participation		p (a)
Age (years)				0.321
16–17	287	286		
18–19	663	290		
20–22	457	316		
Ethnicity			***	0.000
Black, non–Hispanic	731	280		
Hispanic	316	379		
White or other	359	260		
Living arrangement				0.933
Living with mother	484	296		
Not living with mother	908	298		
Number of children				0.927
1	922	297		
More than 1	486	299		
Age at first child's birth (years)				0.456
13–16	574	290		
17–19	834	303		
Age of youngest child (years)				0.833
Less than 1	756	296		
1 or older	650	300		
Educational attainment			***	0.000
No high school diploma or GED	1,314	285		
Had high school diploma or GED	92	477		
Highest grade completed			**	0.025
10th or below	934	284		
11th or above	473	324		
Interval since last attended regular high school			**	0.037
More than 2 years	730	283		
2 years or less	637	319		
TABE reading test score (grade equivalent) (b)			*	0.058
Below 6th grade	295	257		
6th or 7th grade	335	323		
8th or 9th grade	384	301		
10th grade or above	390	305		

(continued)

TABLE 3.3 (continued)

Characteristic and Subgroup at Random Assignment	Sample Size	Average Hours of Participation		p (a)
Ever employed			*	0.075
Yes	1,110	305		
No	298	269		
Prior-year earnings				0.271
$0-$500	1,111	293		
$501 or more	292	316		
Any AFDC received in household				0.706
Yes	1,332	297		
No	75	311		
Family received AFDC when sample member was growing up			***	0.002
Always	242	259		
Sometimes	641	282		
Never	517	335		
CES-D (depression) Scale (c)			**	0.034
0-15 (not at risk)	671	306		
16-23 (at some risk)	352	318		
24-60 (at high risk)	382	262		
Sample size	1,408			

SOURCES: MDRC calculations from New Chance Enrollment Form and MIS data.

NOTES: Calculations for this table used data for 1,408 experimentals for whom there were 18 months of follow-up survey data, including values of zero for those who were randomly assigned to New Chance but did not participate. The table includes 18 months of MIS follow-up data for each individual.

(a) A t-test or F-test was applied to each regression-adjusted difference between subgroup outcomes. The column labeled "p" is the statistical significance level of the difference between subgroup outcomes: That is, p is the probability that subgroup outcomes are different only because of random error. Statistical significance levels are indicated as *** = 1 percent; ** = 5 percent; * = 10 percent.

(b) The test used to measure reading ability was the reading part of the Tests of Adult Basic Education (TABE). Most sites administered the Survey Form of the test, but some administered the full reading test.

(c) The Center for Epidemiological Studies Depression (CES-D) Scale is a widely used measure of depression; scores can range from zero to 60.

women who were 16 or 17 years old participated for 286 hours, those 18 or 19 participated for 290 hours, and those 20 to 22 participated for 316 hours. However, the absence of an asterisk and the p-value of .321, which is greater than .1, both indicate that the differences in average participation hours for these subgroups were not statistically significant and could have arisen by chance alone.[33]

The table shows that higher hours of participation were associated with a number of characteristics generally indicative of being more advantaged, educationally and otherwise: having attended school more recently, having had a high school diploma or GED at baseline, having completed more years of schooling, coming from a family that never received welfare, and not being at high risk of depression. (However, some variables that might be thought to be predictive of lower participation – e.g., number of children – were not.) The data on reading level are less straightforward. They indicate that, as expected, those reading at the eighth-grade level or above attended more hours than the very poorest readers (those reading below the sixth-grade level). However, the highest attendance hours were registered by those who at entry read at the sixth- or seventh-grade level – perhaps because they needed the extra instruction to be able to pass the GED test.

Hispanic young women also had higher participation hours than either their white or black counterparts. This was not simply the result of their having been concentrated at a few sites where participation was relatively high. The enrollee roster at 14 of the 16 sites included young women of Hispanic background; at 9 of these 14, the Hispanic enrollees had more participation hours than women of other ethnic groups, and at 4 of these sites, the difference favoring Hispanics was statistically significant.[34] Nor is it likely that Hispanic young women needed to stay in the program longer because of language difficulties; only 7.0 percent of Hispanic enrollees were judged to speak limited English upon their entry into the research, compared to 3.4 percent of non-Hispanic blacks and 2.9 percent of whites or others (not shown in tables).

C. Site Variation in Participation

Table 3.4 shows how the 16 sites performed on each of six participation indicators. The table makes clear that, on each indicator, the sites' records varied considerably. The proportion participating in any activity, for instance, ranged from 67.1 percent in the Bronx to 100.0 percent in Denver. Similarly, the average number of hours of participation ranged from 122.9 in Chicago Heights to 488.5, again in Denver.[35] Disparities in hours of participation are pronounced for both Phase I and Phase II components.

---

[33] The table presents comparisons based on unadjusted subgroup differences. That is, the analysis does not take into account the fact that some subgroup characteristics might be systematically related to other characteristics (e.g., age and number of children) that might also affect participation.

[34] Although the results are not conclusive because of small subgroup sizes, an analysis of participation among young women of different ethnic groups by site suggests that participation hours tended to be lowest for those ethnic groups that were in a distinct minority at that site. One possible explanation is that, in order for the program environment to feel welcoming enough to invite sustained participation, young women may need to feel that they are part of a critical mass of participants from the same ethnic background.

[35] As noted above, more than 18 months of MIS follow-up data were available for earlier sample entrants. When all these data were used, the average number of hours in skills training increased between 18 and 55 hours in some sites (Allentown, Philadelphia, Pittsburgh, Portland, and San Jose). In Jacksonville and Lexington, average hours in education increased by 24 and 17 hours, respectively, with the longer follow-up.

TABLE 3.4

PARTICIPATION IN NEW CHANCE WITHIN 18 MONTHS AFTER RANDOM ASSIGNMENT, BY SITE

Site	Sample Size	Participated in Any Activity (a) (%)	Average Hours in Phase I Activities (b)	Average Hours in Phase II Activities (c)	Average Hours in All Activities (a)	Participated over 300 Hours (%)	Average (Mean) Months of Activity (a)
Allentown	76	92.1	192.2	79.5	271.7	35.5	6.4
Bronx	88	67.1	121.7	36.3	158.0	19.3	3.1
Chicago Heights	47	78.7	95.3	27.6	122.9	19.2	3.3
Chula Vista	84	85.7	213.6	91.3	304.9	45.2	7.2
Denver	73	100.0	308.2	180.3	488.5	67.1	7.8
Detroit	114	98.3	276.4	64.1	340.6	40.4	6.1
Harlem	86	74.4	189.1	72.2	261.3	33.7	4.7
Inglewood	88	84.1	177.7	93.8	271.5	36.4	6.4
Jacksonville	97	97.9	188.8	68.6	257.4	36.1	6.9
Lexington	95	88.4	211.6	30.3	241.9	31.6	6.2
Minneapolis	82	93.9	167.6	60.9	228.4	24.4	6.0
Philadelphia	89	93.3	219.6	80.5	300.0	33.7	8.0
Pittsburgh	113	89.4	209.4	153.4	362.8	46.0	7.1
Portland	95	95.8	161.6	301.2	462.8	57.9	9.0
Salem	92	84.8	170.4	23.0	193.4	27.2	5.1
San Jose	89	91.0	271.4	134.7	406.1	58.4	8.0
All sites	1,408	88.8	201.8	95.8	297.6	38.8	6.4

SOURCE: MDRC calculations from New Chance MIS data.

NOTES: Calculations for this table used data for 1,408 experimentals for whom there were 18 months of follow-up survey data, including values of zero for those who were randomly assigned to New Chance but did not participate. The table includes 18 months of MIS follow-up data for each individual.

(a) Excludes individual counseling and college classes.

(b) Phase I hours include time spent in adult basic education and GED preparation, career exploration and pre-employment skills training, Life Skills and Opportunities curriculum, health education and health care services, family planning, adult survival skills training, parenting education, and pediatric health services. They exclude time spent in individual counseling.

(c) Phase II hours include time spent in skills training and work internships. They exclude time spent in college and job placement assistance.

Three sites – Denver, Portland, and San Jose – performed better than average on most of the indicators shown. The Bronx, Chicago Heights, and Salem did worse than average.[36]

1. **Participation in Phase I.** Because sites operated Phase I in a relatively uniform fashion, providing similar amounts of program services, disparities in Phase I participation hours reflect factors other than service offerings. One possible explanation for these differences is that some sites enrolled young women who were "easier to serve" – less disadvantaged or more motivated – than others. Indeed, as noted in Chapter 2, baseline characteristics among enrollees at the sites sometimes differed quite dramatically.[37] To test this hypothesis, statistical procedures were used to adjust for these baseline differences; after these adjustments, the numbers did change, but differences among the sites in the average hours of participation remained highly statistically significant (not shown in tables). Furthermore, the classification of sites as better or worse than average did not change. Thus, enrollee characteristics played a fairly modest role in explaining differences in participation.

A second possibility is that differences in Phase I participation hours were driven by differences in the proportion of young women who enrolled in the program but never participated and are shown as having had no hours at all on the MIS. However, when hours of participation in the Phase I components were examined only for experimentals who were ever active in the program, there were still marked differences among the sites, suggesting that nonparticipation does not adequately explain the differences shown in Table 3.4.

Other factors affecting the extent of Phase I participation are hard to identify. In this regard, it is notable that sites with on-site child care did not have appreciably higher Phase I participation hours, on average, than sites without this service; and that sites that operated five days a week had only slightly higher average hours than sites operating just four days a week (leaving the fifth day for participants' appointments and staff planning). It is reasonable to hypothesize that differences in Phase I participation rates may reflect differences in the relative strength or weakness of program services and staff at the different sites, and of their greater or lesser fit with enrollees' needs.

2. **Participation in Phase II.** Differences among the sites in their implementation of the Phase II components account for some of wide variation in the proportion of enrollees participating in these activities. Thus, as shown in Table 3.5, 19.3 percent of enrollees in the Bronx took part in skills training or a work internship, as did 94.7 percent of their Portland counterparts.[38] The proportions of enrollees in skills training or work internships reflect the varying emphasis program operators gave to these components. At several sites, work internships were used only rarely. Staff at the Chicago Heights program, for instance, did not assign anyone to a work internship, reasoning

---

[36] In the Bronx, those assigned to the experimental group sometimes had to wait several weeks for the next cohort to begin. Attrition during these periods accounts in part for the low percentage of women ever active there (67.1 percent), and for the site's poor performance on the other indicators (all of which include both experimentals who were active in New Chance and those who never participated at all and thus had zero hours recorded on the MIS).

[37] For example, only 4.5 percent of the Denver enrollees came from families that had always received welfare; in the Bronx, the proportion was 48.4 percent.

[38] Data on college attendance are not included in the table, since New Chance staff were not required to maintain detailed MIS records on enrollees' attendance in college classes. Job development, another Phase II activity, was largely an activity that program staff undertook on behalf of participants and was not reported on the MIS.

TABLE 3.5

PERCENTAGE AND AVERAGE HOURS OF PARTICIPATION OF NEW CHANCE EXPERIMENTALS IN SKILLS TRAINING OR A WORK INTERNSHIP WITHIN 18 MONTHS AFTER RANDOM ASSIGNMENT, BY SITE

	Skills Training		Work Internship		Skills Training or Work Internship	
Site	Participated (%)	Average Hours	Participated (%)	Average Hours	Participated (%)	Average Hours
Allentown	14.5	45.1	30.3	34.4	34.2	79.5
Bronx	19.3	31.1	3.4	5.2	19.3	36.3
Chicago Heights	21.3	27.6	0.0	0.0	21.3	27.6
Chula Vista	50.0	77.1	9.5	14.2	51.2	91.3
Denver	45.2	155.9	16.4	24.4	45.2	180.3
Detroit	14.9	13.7	21.1	50.5	28.1	64.1
Harlem	73.3	53.7	14.0	18.5	74.4	72.2
Inglewood	40.9	72.1	26.1	21.7	48.9	93.8
Jacksonville	16.5	14.7	33.0	53.9	39.2	68.6
Lexington	9.5	17.9	22.1	12.4	28.4	30.3
Minneapolis	6.1	2.1	24.4	58.8	25.6	60.9
Philadelphia	20.2	48.1	22.5	32.4	29.2	80.5
Pittsburgh	34.5	124.4	25.7	29.0	42.5	153.4
Portland	94.7	245.3	43.2	55.9	94.7	301.2
Salem	10.9	9.3	20.7	13.7	25.0	23.0
San Jose	53.9	127.1	9.0	7.6	57.3	134.7
All sites	33.0	67.4	21.0	28.4	42.0	95.8

SOURCE: MDRC calculations from New Chance MIS data.

NOTES: Calculations for this table used data for 1,408 experimentals for whom there were 18 months of follow-up survey data, including values of zero for those who were randomly assigned to New Chance but did not participate. The table includes 18 months of MIS follow-up data for each individual.

that the participants would not be interested in unpaid work. At the Portland site, in contrast, all students were supposed to hold two work internships (one two weeks in length, the other six) before completing the program, a fact that accounts for relatively high participation in this activity.

Program structure helps explain the unusually high participation in skills training in Harlem and Portland. These sites offered skills training concurrently with education and other activities that at the other sites constituted the program's first phase.[39]

Still other site-specific factors also help to explain the degree to which sites made use of Phase II components. For example, in Inglewood, staff sought to restrict training to those who had already completed their GEDs, having learned from experience that once participants entered training, they rarely completed work toward a GED. In Philadelphia, most training programs funded through the Private Industry Council were reluctant to enroll teens during the period under study, and the New Chance participants were unwilling to commit to longer courses available through local community colleges.

Perhaps the most important factor accounting for site variation in the number of young women entering Phase II activities was site variation in the proportion of young women who earned a high school diploma or GED, a topic discussed extensively in Chapter 5. Table 3.6 shows the proportion of young women who entered a Phase II component at each site, broken down according to whether or not they had received a GED (or high school diploma) by the 18-month follow-up; it also shows the number of months of activity by GED (or high school) completion status. The table makes it clear that at 11 of the 16 sites the majority of high school or GED completers – 64.5 percent of the GED holders across all sites – did go on to skills training or a work internship.[40] (The percentage would be higher if data on college attendance – an activity not recorded on the MIS – were included.) But the overall averages are lowered by the fact that at most sites the majority of young women did not earn a GED, and that only 25 percent of those without a GED or high school diploma entered a Phase II component. Phase II participation was much lower for non-GED earners for two reasons. First, many of these young women dropped out of New Chance early. (On average, those who did not earn a GED were active in the program for 5.0 months; in contrast, GED earners were active for 8.4 months – closer to the length of time anticipated by program planners.) Second, site staff tended to disregard the guideline that non-GED attainers move on to a Phase II activity by the fifth month in cases where young women appeared capable of passing the GED test with additional preparation.

---

[39] In Harlem, the skills training that occurred during the first months of a young woman's program stay might be more accurately termed "pre-training": a typing class attended by all enrollees, the purpose of which was not to qualify the young women for jobs as typists but, rather, to impart a skill useful for whatever paths they subsequently chose. After leaving Phase I, the Harlem participants then moved on to formal skills training programs.

[40] In this regard, it is notable that about one-fifth of the enrollees in Pittsburgh and Portland had received a high school diploma or GED before enrolling in New Chance. In Pittsburgh, these students followed a specialized schedule that emphasized employability development but included all aspects of the model; they were expected to be in this "career group" for two months before entering skills training. Portland also placed these students in classes intended for participants who had passed the GED test, although some were also placed in a GED class to work on specific skills.

TABLE 3.6

PERCENTAGE AND TOTAL AVERAGE MONTHS OF PARTICIPATION OF NEW CHANCE EXPERIMENTALS IN PHASE II COMPONENTS WITHIN 18 MONTHS AFTER RANDOM ASSIGNMENT, BY SITE AND GED RECEIPT AT 18 MONTHS

	Received a GED at 18 Months (a)		Did Not Receive a GED at 18 Months (a)	
Site	Participated in Skills Training or Work Internship (%)	Total Average Months of New Chance Activity	Participated in Skills Training or Work Internship (%)	Total Average Months of New Chance Activity
Allentown	61.3	8.3	15.6	5.2
Bronx	53.9	5.9	13.3	2.6
Chicago Heights	47.4	4.6	3.6	2.4
Chula Vista	78.1	9.3	34.6	6.0
Denver	64.6	8.4	8.0	6.7
Detroit	50.0	9.6	21.6	5.1
Harlem	88.9	7.2	67.8	3.6
Inglewood	77.3	7.9	20.5	4.8
Jacksonville	61.3	8.6	28.8	6.1
Lexington	40.6	6.3	22.2	6.1
Minneapolis	32.1	7.1	13.8	3.8
Philadelphia	64.3	9.4	22.7	7.7
Pittsburgh	56.4	8.6	11.4	3.7
Portland	98.6	11.0	82.6	2.9
Salem	48.5	6.2	11.9	4.4
San Jose	75.0	9.8	32.4	5.6
All sites	64.5	8.4	25.2	5.0

SOURCES: MDRC calculations from survey data and New Chance MIS data.

NOTES: Calculations for this table used data for 1,408 experimentals for whom there were 18 months of follow-up survey data, including values of zero for those who were randomly assigned to New Chance but did not participate. The table includes 18 months of MIS follow-up data for each individual.

(a) Includes a small number of enrollees who received a high school diploma.

How One Program Structured Employment-Related Services

The Portland New Chance site was atypical in three major respects: It represented a collaboration between the Portland Public Schools and the Job Corps; it provided education and skills training concurrently; and it delivered skills training at the program site. Employment-related services in Portland were among the strongest and most thoughtfully structured in the entire New Chance Demonstration, and an examination of how the site assembled the New Chance components may provide insights for other programs as well.

As soon as they enrolled in the program, all students were scheduled to attend vocational classes – typing, computer, and 10-key machine operation – along with GED, parenting, and life skills. The number of periods of GED preparation a young woman was expected to attend was determined by her baseline reading score: A student who scored relatively low was assigned two periods of GED a day; a student with a higher score was assigned only one period. (Students with a GED or high school diploma at entry attended a Business English class.) The clerical component (typing and operation of 10-key machines) had three levels of competency: receptionist, secretary, and word processor. In 1992, the program added legal and medical components as an optional follow-up to completion of the three basic competency levels.

All training coursework was designed to meet minimum standards needed by industry. The instructors were highly experienced, having previously taught in proprietary schools, community colleges, or other adult education programs. They consulted regularly with area employers about the changing skills requirements in office settings.

Students worked independently to complete groups of modules for each class. As a young woman achieved specific benchmarks, her schedule could be changed weekly to move her through the program components. For example, as she completed Business English, she could be rescheduled to add another typing class or computer class. Participants appeared to respond well to this structure: They could work at their own pace, and the modular system allowed them to experience short-term successes.

Portland staff also supplemented the New Chance model with other services they deemed necessary. For example, case managers heard from many young women about incest or physical violence in their homes and, in conjunction with a community agency, set up an "incest survivors" group. Drug abuse and gang violence were issues in the surrounding neighborhood, and a drug users support group with weekly meetings was begun; staff also consulted with a community expert about gang issues. These topics were sometimes addressed in weekly all-participant meetings, sometimes in support groups or life skills classes, and sometimes in one-on-one meetings with case managers.

Instructors and case managers met weekly to discuss the progress and problems of specific students, and attendance was closely monitored. Students were required to sign in and out at the beginning and end of each day; each instructor also took attendance each period. Follow-up on absent students took place the same day, and those who did not call in were considered "AWOL" and did not receive the stipend given to Job Corps enrollees (approximately $2.50 a day) for that day. Poor attenders were asked to meet with program staff. They were permitted to take a leave of absence to resolve personal issues; but participants who continued to miss classes were asked to leave ("resign from") the program.

(continued)

How One Program Structured Employment-Related Services (continued)

Two work internships were built into the skills training courses: one of two weeks' duration in the middle of the course, and the second six weeks long, near the completion of the training. The two internships had different goals. The first was intended to expose students to the workplace environment, to allow them to practice skills learned in classes, and to build up the enthusiasm and motivation of students who were having difficulty finishing the training. The goal for the longer-term internship was job placement: It was intended to provide a "tryout period" during which the employer could evaluate the student – and, if the employer liked what she or he saw, hire her after the internship was completed. While a pool of work internship positions existed, staff also developed specific internships to meet individual needs.

Staff worked closely with students at the job development and placement stage, and students were also referred to a two-week employability skills class.

As students moved from on-site components into jobs, the responsibility for monitoring shifted from their case manager to the "after-care coordinator." Initially, the coordinator contacted each student who was to be followed up every two weeks; over time, the two decided when the young woman no longer needed such frequent follow-up. However, students were urged to drop in and call often, and staff were available to help graduates in whatever way help was needed. Students generally took advantage of these services and only rarely asked to have follow-up stopped. In early follow-up contacts, students often asked for help with various problems, chief among them legal matters, problems with the welfare agency, and securing money for child care. Later, they liked to use the follow-up call just to talk about their situation in general. Many students developed close relationships with program staff members and stayed in touch with them after they left New Chance.

The chief limitation of the program was that it offered only a single type of training in business skills. Prospective enrollees were informed at first contact that the program was for those interested in this area; others were screened out. (If they asked, they were referred to other Job Corps centers.) Recognizing this limitation, program staff have urged the school district to expand the program to include other types of on-site training, to meet the needs of many more young mothers in the area.

Importantly, not all of the young women who completed a GED and were slated to move on to Phase II were interested or felt ready to do so. Program coordinators and other key personnel at some sites (e.g., Chicago and Detroit) commented that many young mothers had enrolled in New Choice with one goal in mind – earning a GED. Having attained this goal, they were much less interested in, or ready to make a commitment to, the program's employment objectives. (To this end, one program coordinator commented that staff should have stressed the importance of the Phase II activities earlier in the participants' program stay.) Other young women felt that they needed and deserved "time off" between receiving their GEDs (an arduous process for some) and moving on to other demanding activities. For some young women, the barrier to forward progress was not lack of motivation, but anxiety – anxiety about encountering new experiences and about leaving the supportive environment of New Chance. Finally, still other young women had unrealistic expectations about the kinds of jobs they would be able to get with a GED and felt that they did not need additional training.

D. Staff Explanations for and Responses to Absenteeism

Some absenteeism is inevitable in any program, and especially in programs serving those who are disadvantaged and whose lack of economic resources restricts their options for dealing with unanticipated problems.[41] In formal and informal interviews with MDRC personnel, program staff cited many reasons for poor attendance. Some believed that their sites had experienced high absenteeism in part because program staff had not enunciated and emphasized clear requirements and expectations from the start. Over time, these sites tried to implement more stringent policies. However, attendance did not improve over time for the group as a whole: Hours of participation for earlier program enrollees (those randomly assigned through September 1990) exceeded those for later entrants. Site-level data indicate that while attendance at some sites improved, at others it got worse, and there is little evidence that efforts to articulate stronger attendance requirements were successful.

At the conclusion of the demonstration, program coordinators were asked to reflect on how they would change program rules and policies; most of the responses pertaining to improving attendance indicated a need for stricter rules. Forcefully articulated rules appear to have been a necessary, but not a sufficient, condition of good attendance. That is, such rules did not result in good attendance at all sites, but all sites where attendance was *not* a particular problem did have clear rules, made them known at the outset of a young woman's stay, and reinforced them periodically through group discussions and other methods.

Along with clear rules, sites adopted other strategies to improve attendance. For one thing, case managers tried to follow up on absenteeism immediately, calling a young woman on the same day to ascertain the reason for her absence, find out whether the program could intervene to resolve any problems, and stress the importance of her being there. For another, the local programs developed a variety of rewards for good attenders, including recognition ceremonies, special lunches in nice restaurants, and points redeemable for items participants valued (e.g., children's clothing or movie tickets).

Site staff also identified a number of obstacles to regular attendance: transportation problems; disruptions in child care arrangements; the young women's illnesses and those of their children;[42] conflicting welfare and medical appointments; enrollees' lack of interest or lack of habituation to a daily routine; and personal problems. The latter could be serious. Reviewing the situations of early program entrants, program staff reported that almost half the young women with whose situations they were familiar did not have a stable place to live at some point during their program tenure, and that smaller but disturbing percentages of enrollees were the victims of physical abuse, used alcohol or illegal drugs (or had family members or partners who did so) to such an extent that this use interfered with their program attendance, or were discouraged from participating by boyfriends or family members.[43]

---

[41] For example, a young woman whose regular ride falls through and who lives far from public transportation cannot simply call a taxi to take her to the program.

[42] Quint and Musick (1994) suggest that pregnancy-related sickness and discomfort were another major reason for absenteeism.

[43] For further details, see Quint, Fink, and Rowser, 1991, pp. 105-10.

As the conclusion of the operations phase of the demonstration approached, program coordinators were asked if they could predict which young women would attend regularly and do well in the program. All but

(continued...)

Program staff reported that participants in Phase II also faced problems that interfered with their program participation. Some of these were issues confronted by Phase I enrollees as well: child care and transportation difficulties, problems with families and housing, and jealousy and opposition on the part of boyfriends. Others were particular to this new stage of the problem. Enrollees often found that college and training program instructors were stricter and less supportive than New Chance staff had been, and the environments in general less congenial; the young women also had difficulty keeping up with the work or managing their time. New Chance staff were sometimes able to help young women transfer to other programs or activities when an initial choice proved inappropriate; in other cases, the young women were able to make the switch on their own; and in still other cases, they simply dropped out.

IV. The Quality of Program-Related Child Care

Free child care is a key element of the New Chance model, intended both to facilitate the young mothers' regular participation in program services and to enhance the development of their children. Regular child care was provided to participants' children at the program site in nine of the 16 program locations.[44] A few sites, notably Inglewood and Jacksonville, made arrangements for the children of New Chance enrollees at family day care homes or day care centers located close to the program facility. Some sites used resource and referral agencies to help participants locate care, while many participants found care on their own.

Because child care was considered an important mechanism for enhancing the development of the New Chance children, the quality of child care provided to them was assessed. As described below, this assessment indicated that the centers generally offered good-quality care, demonstrating the feasibility of integrating good child care into programs for young mothers on welfare.

Data for the assessment were gathered in eight of the 16 sites; the sample included seven on-site centers and four off-site centers. Measures of the quality of child care provided in these centers were derived from two sources. The first was a survey completed by the directors of the child care centers. The survey covered structural aspects of centers that prior research had shown to be correlated with positive child development outcomes (e.g., group size, child-to-staff ratios, training and education of the caregivers, and staff stability). The second source involved observation-based ratings of the overall quality of the child care environment. The ratings were made on the basis of day-long observations in classrooms, using carefully tested observational instruments that have been used in large-scale day

---

[43](...continued)
one said they could. Among the factors coordinators mentioned as predictors of good participation were: a young woman's attendance during the first few weeks, strong support from her family and boyfriend, stable housing, and a high reading level – i.e., conditions that made a young woman more advantaged relative to her peers. It should be noted, however, that the young women who do best in the program need not necessarily be the young women on whom the program registers the largest impacts (i.e., vis-à-vis the control group).

[44] Two sites (the Bronx and Philadelphia) offered temporary care only; a third site, San Jose, had an on-site center, but it was used by few New Chance enrollees because slots were not set aside for them.

care studies.[45] These scales assess quality along such dimensions as personal care routines; language and reasoning experience; fine and gross motor activities; social development; and furnishings and display. Ratings for individual items range from 1 ("inadequate care") to 7 ("excellent care"). Data from the New Chance assessments were compared to several other sources as a means of placing the findings into context.

Table 3.7 presents findings with regard to two important structural characteristics: the child-to-staff ratio and group size of the centers, according to the children's ages. The first two columns show two New Chance averages: averages based on the number of enrolled children, as reported in the survey of center directors, and averages based on actual observed numbers of children in classrooms during the observations.[46] In the third column, the New Chance averages are compared to child care center accreditation criteria established by the National Association for the Education of Young Children (NAEYC) – criteria that are widely considered to reflect expert opinion on child care standards for high-quality care. The last two columns show the averages for two comparison samples of child care centers serving primarily low-income children: first, a subsample from the nationally representative sample of more than 2,000 centers included in the Profile of Child Care Settings (PCS) study (Kisker et al., 1991), and second, a subsample from a study of 227 centers in five U.S. cities in the National Child Care Staffing Study (NCCSS; Whitebrook, Howes, and Phillips, 1990). With respect to child-to-staff ratios, the New Chance ratios (for both enrolled and observed averages) met or exceeded NAEYC standards for children of all ages, and compared favorably with the ratios in both comparison samples. With the sole exception of group size for infants (children under the age of 1) as reported in the survey of directors, the New Chance centers also met or exceeded NAEYC standards for group size and again did well in comparison to the average group size for other centers serving a similar population of children.

With regard to other structural characteristics of the New Chance centers, as reported in the survey of directors, the findings indicate the New Chance child care teaching staff had considerable experience and child care training. Staff turnover in the New Chance centers tended to be lower than turnover in the centers examined in the two comparison studies. However, in comparison with staff in the PCS and NCCSS studies, the New Chance staff had somewhat lower levels of educational attainment.

The average overall quality rating for New Chance preschool classrooms was 4.87, just under a "good" rating on the 7-point ECERS scale. This scale was also used in the NCCSS study, where the average rating for the subsample of centers serving low-income children was 4.48. In the infant and toddler rooms, the average quality rating for the New Chance centers on the ITERS scale was 4.58. This average is also higher than that for the low-income NCCSS centers, which averaged 3.94 for infant rooms and 4.10 for toddler rooms.

---

[45] The Infant and Toddler Environment Rating Scale, or ITERS (Harms, Cryer, and Clifford, 1990), was used to obtain quality ratings in infant and toddler classrooms. The Early Childhood Environment Rating Scale, or ECERS (Harms and Clifford, 1980), was used to measure overall quality in preschool rooms. Two MDRC staff were trained to use these scales. Inter-rater reliability was quite high, at least 90 percent, for both scales in practice sessions.

[46] The discrepancy reflects the fact that attendance in child care centers is rarely 100 percent; the "observed" numbers correspond closely to what the directors reported with respect to typical rates of absenteeism.

TABLE 3.7

AVERAGE CHILD–TO–STAFF RATIOS AND AVERAGE GROUP SIZE FOR NEW CHANCE CHILD CARE CENTERS, ACCREDITATION STANDARDS, AND TWO COMPARISON CHILD CARE SAMPLES

Characteristic and Age Group	New Chance, per Enrollment Data in Survey	New Chance, as Observed by MDRC Staff	NAEYC Accreditation Criteria (a)	Low–Income PCS Sample (b)	Low–Income NCCSS Sample (c)
Child–to–staff ratio (d)					
0–11 months	4	2	4	4	4
12–23 months	5	4	5	6	5
24–35 months	(e)	(e)	6	8	--
36 months or older	7	5	10	9 (f)	8
Group size					
0–11 months	10	7	8	8	9
12–23 months	12	7	12	11	9
24–35 months	(e)	(e)	12	15	--
36 months or older	14	10	20	18 (g)	16

SOURCES: Calculations from New Chance child care center surveys and observational data; special computer run on data from the PCS Study (Kisker et al., 1991); special computer run on data from the NCCSS study (Whitebook, Howes, and Phillips, 1990); and published NAEYC criteria (Bredekamp, 1984).

NOTES: To simplify the presentation, all averages have been rounded to the nearest whole number. Where data are not applicable, dashes are used.

(a) Criteria established by the National Association for the Education of Young Children (Bredekamp, 1984).

(b) A subsample of 84 nonprofit child care centers serving low–income families, drawn from the nationally representative sample of 2,089 centers in the Profile of Child Care Settings (PCS) study.

(c) A subsample of 25 nonprofit child care centers serving low–income families, drawn from the sample of 227 centers from five U.S. cities in the National Child Care Staffing Study (NCCSS).

(d) The numbers shown are the average number of children per staff member (except for the NAEYC criteria, where the numbers shown are the minimum standard for accreditation).

(e) In the New Chance sample, the averages for toddlers aged 12 to 23 months and 24 to 35 months were combined.

(f) In the PCS sample, the average was 9 for preschoolers aged 36 to 47 months; for older preschoolers (48 to 59 months), the average was 10.

(g) In the PCS sample, the average was 18 for preschoolers aged 36 to 47 months; for older preschoolers (48 to 59 months), the average was 17.

In summary, the child care assessment indicated that the New Chance child care centers offered reasonably good-quality care: They generally met experts' standards for group size and child-to-staff ratios, and appeared to be providing a better overall quality of care than that typically available to children from low-income families. Nevertheless, the centers were in need of some improvement on certain dimensions of quality that were assessed.

V. Participants' Assessments of the Program

As part of the 18-month follow-up survey, half the New Chance enrollees, chosen at random, were queried about several aspects of their program experience. They were asked to rate a number of program dimensions using an 11-point scale, where 0 meant "not at all" and 10 "the most possible." The top panel of Table 3.8 shows the averages across all sites in response to seven questions. Ratings for all dimensions were on the positive side, and enrollees were especially likely to feel that New Chance staff members cared about them as people. The young women tended to be more ambivalent, however, about whether they had been expected to spend too much time at the program; the average of the responses to this question was nearer the theoretical midpoint of the scale (5) than was the case with any other question.[47]

The bottom panel of the table shows participants' average ratings of their sites across these seven dimensions. Three sites (Chicago Heights, Denver, and San Jose) were rated highest by participants, while two (Detroit and Inglewood) received ratings lower than the other sites.

Finally, the young women were asked what they had most liked and disliked about the program. They reported especially liking the program staff; the caring, support, and individual attention they received; the other students; and the opportunity the program afforded to meet new people. Over 30 percent of the respondents could think of nothing they disliked about the program. Interestingly, however, the single most disliked aspect of the program, cited by about one in 12 respondents, was, again, the other students (or at least some of them). This suggests that relationships with peers could make the program a very pleasant – or quite unpleasant – place to be.

VI. The Cost of New Chance

This section presents estimates of the cost of New Chance at each of the 16 demonstration sites.[48] New Chance operated as a collaborative effort between the sponsor agency and other community organizations and funders. Sponsor agencies coordinated the whole program, but did not provide or fund all the services.

These are estimates of *gross* costs: They do not subtract the costs of education, training, and other services received by the control group. The *net* cost of New Chance (i.e., above and beyond expenditures for controls) will likely be considerably lower than the gross cost at many sites because,

---

[47] It is also possible that the wording of this question was confusing to some young women. Whereas more favorable assessments of the program on the other dimensions were associated with *higher* scores on the 0 to 10 scale, in this case, a young woman who felt she was *not* required to spend too much time at the program needed to indicate this with a *lower* rating.

[48] These costs and the procedures by which they were estimated are discussed in detail in Fink, 1994.

TABLE 3.8

EXPERIMENTALS' RATINGS OF NEW CHANCE PROGRAM FEATURES AND SITES

Program Feature and Site	Average (Mean) Rating
General program features (a)	
How much do you think the staff cared about you as a person?	8.0
How much did your case manager help you to get services or other things when you needed them?	7.6
How much did the program help you to achieve your personal goals?	6.8
How much do you feel that you were expected to spend too much time at the program?	4.3
How much did you learn in the educational classes?	7.5
How much did you learn in the employment-related activities?	7.2
How much did the program help you with being a parent?	7.0
Average for 7 program features, by site (b)	
Allentown	7.4
Bronx	6.7
Chicago Heights	7.9
Chula Vista	7.3
Denver	8.1
Detroit	6.1
Harlem	6.9
Inglewood	5.7
Jacksonville	7.8
Lexington	7.0
Minneapolis	7.4
Philadelphia	7.1
Pittsburgh	7.5
Portland	6.7
Salem	7.1
San Jose	8.0
All sites	7.1
Sample size	582

SOURCE: MDRC calculations from survey data.

NOTES: Calculations for this table used data for 582 randomly selected experimentals for whom there were 18 months of follow-up survey data and who were asked the questions.

(a) Ratings were on an 11-point scale, where 0 meant "not at all" and 10 meant "the most possible."

(b) In calculating this mean, the average score for "How much do you feel that you were expected to spend too much time at the program?" was inverted to achieve consistency with the other scores, where a higher number indicated a more positive rating.

as discussed in Chapter 4, many controls received similar services. Both the costs of control group services and the net costs of New Chance will be included in the benefit-cost analysis to be presented in the final report.

Table 3.9 disaggregates New Chance costs along a number of dimensions. First, it distinguishes between services financed by sponsor agencies and those funded by other organizations at no cost to the New Chance sponsor.[49] In addition, it presents New Chance costs for three broad categories: Phase I services, Phase II services, and child care.[50] The cost borne by sponsor agencies in providing Phase I and Phase II services averaged $5,073 per experimental. At a minimum, sponsor agencies financed case management, recruitment and intake, and program management functions. They also drew on other agencies to provide New Chance services. For example, in several sites the public school system paid for basic education instruction (even though the instructor was stationed at the New Chance site).[51] Other agencies spent an average of $1,380 per experimental on New Chance services.

Table 3.9 shows that some sites (Allentown, the Bronx, Chicago Heights, and Chula Vista) relied almost exclusively on the New Chance sponsor agency for funding Phase I and Phase II activities. In other cases, the cost to sponsor agencies of running New Chance was considerably lower than the total value of services provided, with non-sponsor agencies bearing up to 51 percent of Phase I and Phase II costs.[52]

In terms of costs by service categories, Phase I services accounted for more than half (57 percent) of total costs, child care for 29 percent,[53] and Phase II services (primarily occupational skills training and college) for 15 percent. Phase II would have been more costly if participation in these activities had been higher.

Taking into account both expenditures made by the sponsor agencies and the value of services provided by outside organizations at no cost to the sponsor agencies, the total cost of New Chance averaged $9,026 per experimental. The table reveals substantial variation by site in the absolute level of expenditures and the composition of total costs. The total cost of New Chance ranged from $4,758 in Chicago Heights to $16,846 in Portland.

---

[49] Sponsor agency costs refer to all expenditures charged against a budget of the New Chance sponsor agency.

[50] In this analysis, child care costs include the cost of center-based and family day care (the latter is provided for a small group of children in a caregiver's home). For New Chance participants, these services were paid for largely with JOBS funds, and so the costs could be estimated using JOBS child care funding formulas. It was not possible to reliably estimate the costs of care by friends or relatives because information was lacking about how much JOBS (or participants themselves) contributed for this type of care.

[51] As another example, personnel from agencies specializing in women's health issues often provided family planning instruction. Even when a staff member from such an agency provided time at no cost to the New Chance sponsor, the estimated value of her time (i.e., a portion of her salary) was included as an "other agency" cost in calculating the total cost of New Chance.

[52] The line between sponsor agency and non-sponsor agency costs is sometimes thin. The distinction hinges on whether funds were channeled through the New Chance sponsor agency or the outside agencies provided services at no cost to the sponsor agency.

[53] All case management costs were included in the cost of Phase I.

TABLE 3.9

TOTAL COST OF NEW CHANCE PER EXPERIMENTAL, BY SITE, AGENCY, AND PROGRAM PHASE

	Phase I			Phase II			Phase I and Phase II			Child Care	
Site	Sponsor Agency Cost of Phase I ($)	Other Agency Cost of Phase I ($)	Total Cost of Phase I ($)	Sponsor Agency Cost of Phase II ($)	Other Agency Cost of Phase II ($)	Total Cost of Phase II ($)	Sponsor Agency Cost of Phase I & II ($)	Other Agency Cost of Phase I & II ($)	Total Cost of Phase I & II ($)	Total Cost of Child Care ($)	Total Cost of New Chance ($)
Allentown	8,119	20	8,139	117	492	609	8,236	512	8,748	2,156	10,904
Bronx	4,144	51	4,195	586	81	667	4,730	132	4,862	1,384	6,246
Chicago Heights	3,592	0	3,592	554	356	910	4,146	356	4,502	256	4,758
Chula Vista	4,247	12	4,259	947	301	1,248	5,194	313	5,507	3,034	8,541
Denver	3,961	425	4,386	731	727	1,458	4,692	1,152	5,844	1,788	7,632
Detroit	3,058	2,829	5,887	77	410	487	3,135	3,239	6,374	2,059	8,433
Harlem	4,444	1,295	5,739	616	283	899	5,060	1,578	6,638	3,197	9,835
Inglewood	4,823	1,592	6,415	682	834	1,516	5,505	2,426	7,931	2,826	10,757
Jacksonville	3,501	385	3,886	90	604	694	3,591	989	4,580	2,724	7,304
Lexington	4,695	222	4,917	202	883	1,085	4,897	1,105	6,002	4,458	10,460
Minneapolis	5,201	363	5,564	464	434	898	5,665	797	6,462	4,387	10,849
Philadelpha	3,902	184	4,086	247	578	825	4,149	762	4,911	1,659	6,570
Pittsburgh	5,535	51	5,586	350	1,452	1,802	5,885	1,503	7,388	2,050	9,438
Portland	5,613	2,090	7,703	3,480	621	4,101	9,093	2,711	11,804	5,042	16,846
Salem	2,752	1,091	3,843	155	343	498	2,907	1,434	4,341	1,458	5,799
San Jose	3,738	213	3,951	545	2,850	3,395	4,283	3,063	7,346	2,694	10,040
All sites	4,458	676	5,134	615	703	1,318	5,073	1,380	6,453	2,573	9,026

SOURCES: MDRC calculations from site and MDRC fiscal, administrative, and MIS data.

NOTES: Estimates in this table used data for all 1,408 experimentals for whom there were 18 months of follow-up survey data, including values of zero for those who were randomly assigned to New Chance but did not participate.

All costs are in 1991 dollars. Rounding may cause slight discrepancies in sums.

135

134

The average cost of Phase I activities was $5,134, ranging from $3,592 in Chicago Heights to $8,139 in Allentown. Several factors account for this variation: the length of time experimentals took part in these activities, the number of participants served, staffing arrangements, the scope of activities and services, and overhead costs. Phase II costs averaged $1,318 per experimental across the sites. The variation – from $487 per experimental in Detroit to $4,101 in Portland – was primarily owing to different rates of participation in skills training and college. Portland provided skills training concurrently with Phase I services and had the highest participation by far in skills training activities.

Child care cost an average of $2,573 per experimental across all sites and was funded mostly through JOBS, although programs offering on-site care needed supplemental funds from other sources. The costs were low in Chicago Heights ($256 per experimental) because 85 percent of the experimental group in that site utilized kinds of care not included in this analysis (i.e., care by friends or relatives). Portland's costs ($5,042 per experimental), at the other end of the spectrum, resulted from almost 40 percent of experimentals having used center-based care, coupled with the highest use of family day care among the sites (56 percent).

Table 3.10 shows for each site the combined sponsor agency and other agency costs of the separate components and activities that comprise New Chance, and Figure 3.1 shows the percentage distribution of the cost of each component across all sites.[54] The three most costly components by far – those where the most "staff hours" were spent – were child care, case management, and basic education. Together, they accounted for nearly three-fourths of the program's total cost. As shown in Figure 3.1, the components that made New Chance a comprehensive program were relatively inexpensive: health and personal development services (health education, family planning education, life skills classes, and adult survival skills workshops) together constituted only 5 percent of the total cost of New Chance, while parenting education and employability development each accounted for 3 percent of these costs. It is not surprising that basic education consumed a relatively large share of costs; it was typically provided for three to four hours daily, whereas the other services were usually scheduled once a week.

Skills training and college also accounted for a small share of the program's total cost (8 and 3 percent, respectively) because relatively few experimentals participated in these activities. The cost of skills training varied widely across the sites, from $62 per experimental in Minneapolis to $3,413 in Portland. As noted above, the most important influence on these costs was the extent of participation. Participation rates also affected college costs, which ranged from $50 per experimental in Portland, where few participants attended college, to $1,065 per experimental in San Jose.

* * *

Table 3.11, a compilation of data presented in other tables in this chapter, allows the reader to examine the relationships among selected implementation variables at the site level. It indicates that two of the three sites participants rated most highly (Denver and San Jose) also registered higher-than-average participation; the third (Chicago Heights), however, did not. Neither the relationship between cost and participation nor that between cost and participants' ratings appears to be straightforward.

---

[54] It was beyond the scope of this analysis to estimate the cost of all medical care received by New Chance participants or their children. Thus, the cost of medical care was not included in these estimates.

TABLE 3.10

COST OF NEW CHANCE COMPONENTS PER EXPERIMENTAL, BY SITE

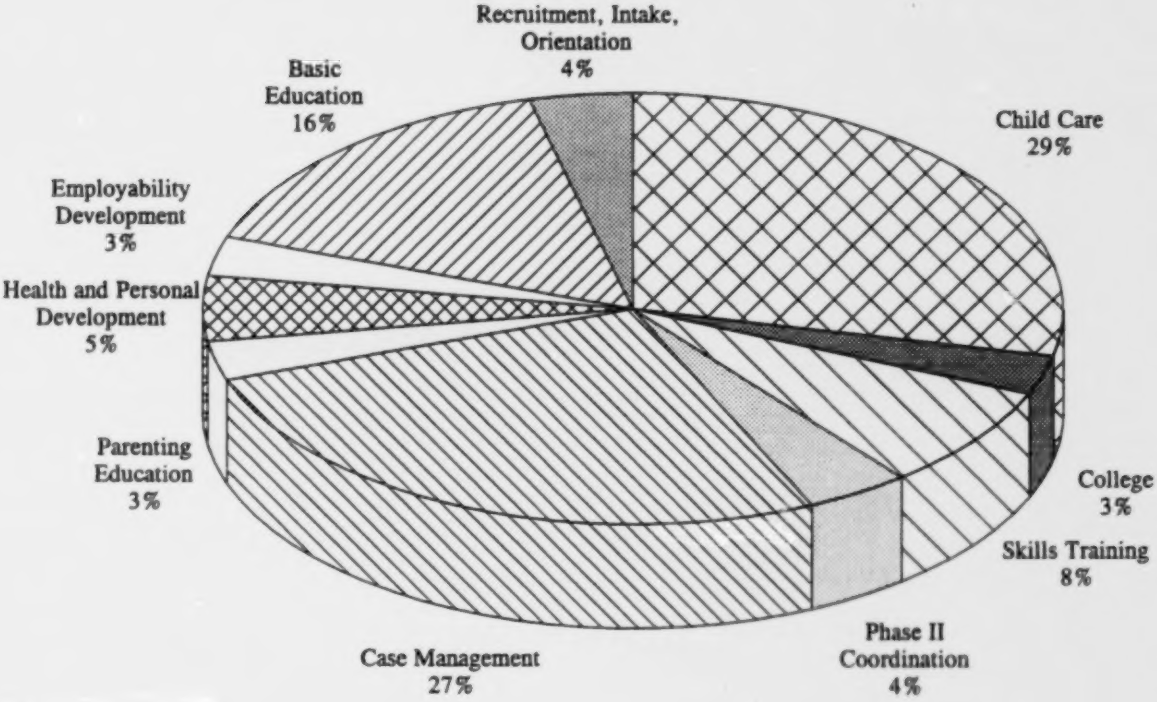
Site	Recruitment, Intake, Orientation ($)	Basic Education ($)	Employability Development ($)	Health and Personal Development ($)	Parenting Education ($)	Case Management ($)	Phase II Coordination ($)	Skills Training ($)	College ($)	Child Care ($)	Total Cost of New Chance ($)
Allentown	118	2,064	133	1,473	967	3,384	75	210	324	2,156	10,904
Bronx	224	962	141	195	112	2,561	407	187	73	1,384	6,246
Chicago Heights	141	980	263	210	125	1,873	555	256	99	256	4,758
Chula Vista	455	808	64	305	110	2,517	523	604	121	3,034	8,541
Denver	160	1,426	133	550	512	1,605	169	889	400	1,788	7,632
Detroit	393	2,506	299	475	318	1,896	81	188	218	2,059	8,433
Harlem	294	1,177	210	217	64	3,777	194	526	179	3,197	9,835
Inglewood	915	1,917	602	1,063	91	1,827	569	346	601	2,826	10,757
Jacksonville	82	934	51	234	106	2,479	59	345	290	2,724	7,304
Lexington	694	2,051	185	191	128	1,668	524	120	441	4,458	10,460
Minneapolis	378	1,192	141	343	476	3,034	443	62	393	4,387	10,849
Philadelpha	235	1,238	95	78	38	2,402	247	524	54	1,659	6,570
Pittsburgh	291	1,498	430	394	109	2,864	350	1,336	116	2,050	9,438
Portland	277	1,335	611	612	754	4,114	638	3,413	50	5,042	16,846
Salem	422	1,875	331	122	88	1,005	128	205	165	1,458	5,799
San Jose	138	583	182	301	177	2,570	570	1,760	1,065	2,694	10,040
All sites	326	1,409	242	423	261	2,474	346	686	287	2,573	9,026

SOURCES: MDRC calculations from site and MDRC fiscal, administrative, and MIS data.

NOTES: Estimates in this table used data for all 1,408 experimentals for whom there were 18 months of follow-up survey data, including values of zero for those who were randomly assigned to New Chance but did not participate.

All costs are in 1991 dollars.

FIGURE 3.1
PERCENTAGE DISTRIBUTION OF THE COST OF NEW CHANCE COMPONENTS



NOTE: Distribution does not total 100.0 percent because of rounding.

TABLE 3.11

SUMMARY OF NEW CHANCE IMPLEMENTATION, BY SITE

Site	Sample Size	Average Hours in All Activities (a)	Average Months of Activity (b)	Enrollee Rating (c)	Average Cost per Experimental ($)
Allentown	76	271.7	6.4	7.4	10,904
Bronx	88	158.0	3.1	6.7	6,246
Chicago Heights	47	122.9	3.3	7.9	4,758
Chula Vista	84	304.9	7.2	7.3	8,541
Denver	73	488.5	7.8	8.1	7,632
Detroit	114	340.6	6.1	6.1	8,433
Harlem	86	261.3	4.7	6.9	9,835
Inglewood	88	271.5	6.4	5.7	10,757
Jacksonville	97	257.4	6.9	7.8	7,304
Lexington	95	241.9	6.2	7.0	10,460
Minneapolis	82	228.4	6.0	7.4	10,849
Philadelphia	89	300.0	8.0	7.1	6,570
Pittsburgh	113	362.8	7.1	7.5	9,438
Portland	95	462.8	9.0	6.7	16,846
Salem	92	193.4	5.1	7.1	5,799
San Jose	89	406.1	8.0	8.0	10,040
All sites	1,408	297.6	6.4	7.1	9,026

SOURCES: MDRC calculations from data collected for the cost analysis and from survey data.

NOTES: (a) Excludes individual counseling and college classes.

(b) Number of months (not necesarily continuous) in which counted enrollees took part in New Chance activities.

(c) The enrollee site rating was calculated as the grand mean of the average scores experimentals at a given site gave to each of the seven dimensions rated in the top panel of Table 3.8. Ratings were on an 11-point scale, where a higher number indicated a more positive rating.

140

Portland operated the most costly program and also had high participation; but other programs with high participation were much less expensive. Sites that participants rated most highly and those they rated least favorably were all close to or below the average cost for all sites.

The extent of participation in program services and the quality of these services are potentially important for explaining program impacts. The chapters that follow return to this theme and examine it more closely.

CHAPTER 4

SERVICE RECEIPT BY EXPERIMENTAL AND CONTROL GROUP MEMBERS

I. Introduction

The impact analysis rests on the assumption that experimentals will receive a significantly larger "dose" than controls of the services that are expected to help them advance toward self-sufficiency and toward better lives for themselves and their children. The validity of this assumption is especially important in mostly voluntary programs such as New Chance because such programs can be expected to generate impacts only on those who participate in them, and only to the extent that participants receive more services than their control group counterparts. (In contrast, mandatory programs for welfare recipients may have impacts on nonparticipants' employment rates and welfare receipt – i.e., the length of time they remain on welfare and the size of their welfare grants – if the mandate induces them to seek and find jobs on their own, or if noncompliance with the program results in a reduction of their grants.) In short, as far as service receipt is concerned, more was likely to be better if experimentals as a group were to register statistically significant impacts vis-à-vis controls with regard to educational attainment and other outcomes; and impacts were likely to be attenuated if controls received services that were substantially similar in kind and amount to those received by New Chance enrollees.[1]

That those who were randomly assigned to New Chance (i.e., the experimentals) would actually receive more services than controls was not a foregone conclusion, for three reasons. First, most applicants to New Chance – those who became controls as well as those who became experimentals – volunteered for the program, meaning that, at least when they applied, they were sufficiently motivated to seek the services offered by the program. The fact that the two groups were created by random assignment means that they were similar even in such unmeasured characteristics as motivation. In other words, controls, as a group, were as motivated as experimentals, as a group, to pursue services they wanted or needed, and were free to seek them through other programs.

Second, New Chance did not have a monopoly on the services it offered. In every New Chance community, there were other programs and agencies from which young mothers could obtain services. Local public school systems and community organizations offered basic education and GED classes at minimal cost or free of charge; community colleges offered a variety of vocational programs. Students could also apply to federal student aid programs for loans to take courses at community colleges or private vocational schools. Family planning services were available from community health agencies and clinics, and community organizations might provide help with parenting and with personal counseling. Controls, however, were unlikely to seek or to obtain on their own the full complement of New Chance services.[2]

---

[1] At the individual level, "more" was not necessarily "better." A young woman with a high reading score at baseline, for instance, was likely to need fewer hours of instruction before passing the GED test than her counterpart with weaker skills.

Further, this discussion assumes that services were not stigmatizing to recipients, marking them as deficient or in need of remediation of some kind, and thereby possibly reducing their attractiveness to potential employers.

Finally, because New Chance operated primarily on a voluntary basis, site staff could seldom use the threat of a welfare sanction to require experimentals to fulfill a minimum participation requirement, or to participate at all; they could offer only inducements and the threat of termination from the program – an action often undertaken only reluctantly.

A. The Scope of This Chapter

This chapter uses data from the 18-month survey to compare service receipt by experimentals and controls.[3] The next three sections examine the extent to which sample members received three critical kinds of services: education services, services related to employment, and services to enhance the young women's personal development and parenting skills. Service receipt for the sample as a whole is reported in two ways: as the percentages of experimental and control group members who *ever* received a given service within the 18-month follow-up period, and as the *amount* of such services received.[4] For selected services, experimental-control differences (i.e., impacts) are presented for subgroups of the sample defined by their demographic and socioeconomic characteristics at random assignment, and for the 16 individual sites. The chapter's fifth section discusses sample members' ratings of the services they received. The last section puts these findings in context by examining service receipt in New Chance and in the other programs serving young mothers that were described in Chapter 1.

B. A Preview of the Findings

In essence, the data make it clear that the impact analysis tested a service *increment*, not the effect of services compared with no services at all. Over the course of the 18-month follow-up period, experimental group members received significantly higher amounts of all services than did their control group counterparts; differences were especially large with regard to personal development and parenting services. However, with respect to human capital development services (education and employment-related activities), these differences, while still statistically significant, were smaller; in fact, over half of the controls participated in education activities of some kind. As expected, differences in service receipt between the groups were most pronounced during the early months after random assignment and steadily diminished over time, as experimentals left New Chance and controls entered other education and job training programs; at the 18-month point, members of the two groups were equally likely to be participating in education or other job preparation activities.

---

[2] As noted previously, New Chance program features – e.g., case management and the fact that most services were delivered in one location – were intended to facilitate service receipt.

[3] Both Chapters 3 and 4 present participation measures for experimentals. However, these measures were based on different data sources. The outcomes presented in Chapter 3 were based on MIS data collected by site staff and reported to MDRC, while all the participation measures discussed in this chapter were drawn from the 18-month survey and based on sample members' self-reports. For several reasons, the survey data revealed somewhat higher rates and durations of participation than did the MIS. First, in order to obtain comparable data for experimentals and controls, the survey captured *all* participation in various activities, whatever their source, while the MIS data were limited to activities directly related to the New Chance program. Also, certain New Chance activities that were reported separately in the MIS data (e.g., GED preparation and life skills classes) appear to have been reported as a single activity – education – by some survey respondents, thus inflating the apparent intensity of education services.

[4] For education services and for some employment-related activities, data were also available on the *intensity* of the services, as measured by the number of hours for which they were scheduled.

II. Impacts on Participation in Education Activities

A. Aggregate Impacts

As the left-hand section of Table 4.1 shows, experimentals were significantly more likely than controls (85.3 percent versus 60.4 percent) to have attended an education program – e.g., adult basic education or GED classes, high school, or college – during the 18-month (six-quarter) follow-up period. But it is also striking that well over half of the controls received some education services. Figure 4.1 shows that the difference between the two groups was, predictably, largest during the first three months after random assignment (50.3 percentage points), when most New Chance enrollees attended GED preparation or basic education (pre-GED) classes. While remaining statistically significant, this difference steadily narrowed through the first five quarters after random assignment as experimentals left New Chance. (Controls' rates of participation in education did not vary much from one quarter to another.) By the sixth post-random assignment quarter, experimentals were slightly more likely than controls to be participating in education programs, but the difference was no longer statistically significant. A similar percentage – about 20 percent – of both experimentals and controls were attending an education program in the month prior to the interview.

A large majority of those who participated in education activities attended GED or basic education classes: 79.4 percent of experimentals and 47.1 percent of controls. The difference between the two groups was most pronounced during the first four quarters after random assignment, but it remained statistically significant in the last two quarters as well. At the 18-month point, less than 10 percent of the young mothers in either group were engaged in GED preparation; controls (9.8 percent) were slightly more likely than experimentals (7.7 percent) to be attending such classes, but the difference was not statistically significant.

Unlike its marked effect on GED participation, New Chance had no impact on high school attendance. Given the young mothers' ages and the length of time they had been out of school (more than two years, on average) – as well as the program's emphasis on GED attainment – it is not surprising that only a handful of young women in either group, but significantly fewer experimentals than controls, chose to enroll in regular high school programs.

Experimentals were significantly more likely to attend college than were controls, and this difference held up throughout the follow-up period. Overall, about one in eight experimentals (12.5 percent), but only one in 13 controls (7.9 percent) attended college at some point during the 18 months after random assignment.[5] About 20 percent of both experimentals and controls attended "other

---

[5] As noted in Chapter 1, college attendance was not part of the formal New Chance model, but was a post-GED activity for a number of young women.

TABLE 4.1

IMPACTS OF NEW CHANCE ON PARTICIPATION IN EDUCATION PROGRAMS WITHIN 18 MONTHS AFTER RANDOM ASSIGNMENT

Activity and Follow-Up Period	Participated in Activity				Average Number of Weeks Participated				Average Scheduled Hours per Week for Those Who Participated in Activity (a)	
	Experimentals (%)	Controls (%)	Difference	p (b)	Experimentals (%)	Controls (%)	Difference	p (b)	Experimentals	Controls
Any education program (c, d)										
Quarters 1-6 (e)	85.3	60.4	24.9 ***	0.000	25.7	13.9	11.8 ***	0.000	20.3	20.0
Quarters 1-4	81.7	48.4	33.3 ***	0.000	20.6	9.6	10.9 ***	0.000	20.4	20.2
Quarters 5-6	38.1	33.1	5.0 **	0.025	5.1	4.2	0.9 **	0.023	19.9	20.2
Attending any education program at follow-up	15.5	16.4	-0.8	0.621	-- (f)	--	--	--	--	--
Basic education/GED (c)										
Quarters 1-6	79.4	47.1	32.4 ***	0.000	20.7	8.7	12.0 ***	0.000	19.7	18.1
Quarters 1-4	76.3	37.4	39.0 ***	0.000	17.7	6.1	11.5 ***	0.000	19.9	18.4
Quarters 5-6	25.6	21.8	3.9 **	0.048	3.1	2.6	0.5	0.103	18.4	17.6
Attending basic education/ GED at follow-up	7.7	9.8	-2.0	0.115	--	--	--	--	--	--
High school										
Quarters 1-6	2.5	3.6	-1.1	0.159	0.6	0.8	-0.2	0.365	22.7	22.0
Quarters 1-4	1.9	2.6	-0.7	0.264	0.4	0.5	-0.1	0.513	26.0	24.2
Quarters 5-6	1.2	1.7	-0.4	0.436	0.1	0.2	-0.1	0.254	31.0	27.5
Attending high school at follow-up	0.7	0.9	-0.2	0.558	--	--	--	--	--	--
College										
Quarters 1-6	12.5	7.9	4.6 ***	0.001	3.3	2.1	1.1 **	0.019	19.0	20.7
Quarters 1-4	9.1	5.8	3.3 ***	0.007	1.8	1.3	0.5	0.101	19.2	20.0
Quarters 5-6	9.6	5.7	3.9 ***	0.002	1.5	0.8	0.6 ***	0.005	18.9	21.8
Attending college at follow-up	5.2	3.3	1.9 **	0.048	--	--	--	--	--	--
Other education program										
Quarters 1-6	20.1	17.8	2.3	0.195	5.2	4.9	0.3	0.626	19.3	21.0
Quarters 1-4	15.0	13.3	1.7	0.279	3.1	3.3	-0.2	0.589	19.0	21.0
Quarters 5-6	14.4	11.7	2.7 *	0.090	2.2	1.6	0.5 **	0.048	18.7	21.1
Attending other education program at follow-up	2.7	3.3	-0.6	0.477	--	--	--	--	--	--
Sample size	1,408	680			1,408	680				

(continued)

TABLE 4.1 (continued)

SOURCES: MDRC calculations from New Chance Enrollment Form and survey data.

NOTES: Calculations for this table used data for all 2,088 sample members for whom there were 18 months of follow-up survey data, including those with values of zero for outcomes and New Chance enrollees (i.e., experimentals) who did not participate in the program.

The averages or percentages are adjusted using linear analysis of covariance procedures controlling for up to 51 kinds of difference in characteristics before random assignment. Rounding may cause slight discrepancies in sums and differences.

(a) These columns contain data only for those who participated in a given activity rather than for the full research sample.

(b) A two-tailed t-test was applied to each regression-adjusted difference between average experimental and control group outcomes. The column labeled "p" is the statistical significance level of the difference between experimental and control group outcomes: That is, p is the probability that average outcomes are different only because of random error. Statistical significance levels are indicated as *** = 1 percent; ** = 5 percent; * = 10 percent.

(c) For controls, services were obtained at programs or agencies other than New Chance. For experimentals, the services were obtained either at New Chance or, if they were served by additional programs, elsewhere.

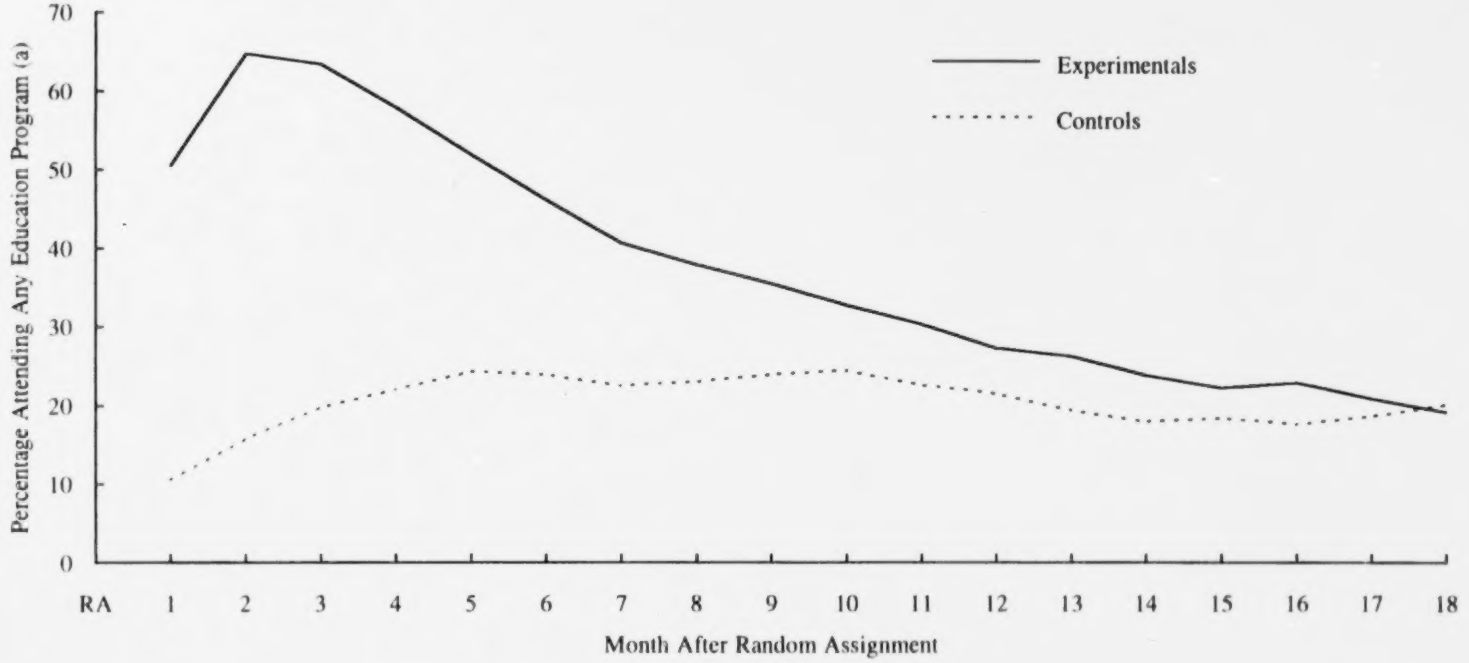
(d) Includes adult basic education, GED preparation, high school, college, and other education programs (e.g., proprietary schools).

(e) Quarter 1 refers to the three calendar months beginning with the month in which the sample member was randomly assigned to the experimental or control group. Thus, e.g., for a young woman who was randomly assigned on May 16, 1990, quarter 1 means the period from May 1 through July 31, 1990.

(f) Where data are not applicable, dashes are used.

FIGURE 4.1

PERCENTAGE OF EXPERIMENTALS AND CONTROLS ATTENDING ANY EDUCATION PROGRAM, BY MONTH AFTER RANDOM ASSIGNMENT



SOURCE: MDRC calculations from New Chance Enrollment Form and survey data.

NOTES: The percentages are adjusted using linear analysis of covariance procedures controlling for up to 51 kinds of difference in characteristics before random assignment.

(a) Includes adult basic education, GED preparation, high school, college, and other education programs (e.g., proprietary schools).

education" classes – activities that the respondents identified as "college" classes rather than as skills training, but that took place at proprietary schools that offered training for specific occupations. (Such institutions frequently call themselves colleges.) It seems likely, therefore, that "other education" classes were more vocational than academic in their thrust. As noted in Chapter 3, however, many of the young women who attended regular two-year colleges also took many vocationally focused classes at those colleges.[6] The young women's distinction between college and skills training often referred more to the type of institution providing the training and to the duration of the course of study (with college taking longer to complete) than to the content of the classes.

The central section of Table 4.1 indicates the average number of weeks experimentals and controls actually attended a given education activity. These weeks were not necessarily continuous: Up to six separate spells of participation were reported. The number of weeks shown for each group averaged together both sample members who never participated (and whose number of weeks in the activity was therefore zero) and those who did participate; because all sample members were included, the difference between the groups represents a true program impact. The right-hand section of the table, in contrast, indicates the average number of hours per week for which sample members who participated in a given activity reported that it was scheduled (not the number of hours they actually attended). These data offer an interesting perspective on the time demands these activities placed on the young mothers. Because the data excluded nonparticipants, however, the differences between the groups cannot be taken as true program impacts because the characteristics of experimentals who did not participate may have been different from those of controls who did not participate.

The table makes clear that experimentals attended education programs for significantly more weeks than did controls. On average, experimentals spent over twice as many weeks in basic education or GED preparation as their control group counterparts during the 18 months of follow-up (20.7 versus 8.7 weeks, respectively), although during the last six months (quarters 5 and 6), the difference was no longer statistically significant. Experimentals also spent more time in college than did controls; here, the disparity between the groups was greatest during the last six months.

The data on hours of scheduled activities pose problems of interpretation because it is not clear that the same thing was being measured for experimentals as for controls. According to the table, experimentals reported having been scheduled for 19.9 hours of GED or basic education classes a week and controls for 18.4 hours during the first four quarters (when experimentals were most likely still to have been in New Chance). Yet, the large majority of the New Chance sites scheduled 10 to 15 hours a week, or two to three hours a day, of such classes, with the remainder of the time having been devoted to employment-oriented or personal and family development activities (see Chapter 3). It seems likely that many experimentals thought of New Chance as primarily a GED program and did not draw a clear distinction between the number of hours they were scheduled to attend *all* New Chance activities versus their scheduled hours in education classes only (despite survey interviewers' instructions that they do so). Thus, they may have inflated the number of hours they were scheduled to attend GED or basic education classes, while controls reported their scheduled hours of GED or basic education classes more accurately. If this is so, then controls may have enrolled in education programs that were at least as intensive, if not as wide-ranging in their focus, as the education activities in New Chance.

---

[6] Thus (not shown in tables), over a quarter of the experimentals who attended college and about a third of their control group counterparts said that they were preparing for office occupations (such as secretary, clerk, or computer operator), and 10 percent of the experimentals and 20 percent of the controls were preparing to become nurse's aides or licensed practical nurses (LPNs).

B. Impacts for Subgroups

Factors other than research status (i.e., membership in the experimental or control group) affected the amount of service sample members received. These included the demographic, socioeconomic, and school-related characteristics of sample members at baseline (i.e., at random assignment).

Table 4.2 is the first of several tables in this report in which impacts are presented for key subgroups of the research sample, defined by their baseline characteristics. All these tables follow a common format, and all address two questions: Within which subgroups were experimental-control differences – i.e., program impacts – statistically significant? Were impacts markedly larger for some subgroups than for others?

The experimental-control differential in the number of weeks of participation in education programs is shown in the column of the table labeled "within-subgroup impact," while the probability that the observed difference between experimentals and controls within the subgroup in question could have arisen by chance, or simply reflects random errors of measurement, appears in the adjacent column labeled "p."[7] Within every subgroup except the small subgroup comprising young women who had a high school diploma or GED when they entered the research sample (many of whom moved quickly into job training or other activities), experimentals participated in education programs for significantly longer periods than did controls. For example, Hispanic young women in the experimental group participated in such programs an average of 29.1 weeks, while Hispanic controls participated an average of 13.4 weeks; experimentals who were black participated on average for 26.1 weeks, while black controls participated for 14.3 weeks.

Determining impacts (i.e., experimental-control differences) for individual subgroups is part of the story. Also of interest is whether the impacts for the individual subgroups (such as each of the three subgroups based on age) in a category (such as age) were themselves statistically significantly different from one another. For example, the column labeled "between-subgroups impact difference" shows the difference between the impacts registered by subgroups within a category, and the "p" column to the right of the category shows the probability that the difference between subgroup impacts was statistically significant – i.e., was unlikely to have arisen by chance or because of measurement error.[8] (Differences that are not statistically significant, as indicated by p-values greater than 0.1, indicate that the program did not have notably larger impacts for one subgroup than for another.) Differences in the magnitude of the program impact were notable for several subgroups: New Chance

---

[7] A fuller explanation of p-values is given in Section VI of Chapter 2.

[8] The between-subgroups impact difference is calculated for characteristics for which there are only two subgroups (e.g., receipt of a high school diploma or GED). For characteristics for which there are more than two subgroups (such as age or ethnicity), the concept of a between-subgroups impact different does not apply, but the probability that the difference among all the subgroups was statistically significant is reported in the table.

TABLE 4.2

IMPACTS OF NEW CHANCE ON NUMBER OF WEEKS PARTICIPATED IN ANY EDUCATION PROGRAM WITHIN 18 MONTHS AFTER RANDOM ASSIGNMENT, BY SUBGROUP

Characteristic and Subgroup at Random Assignment	Sample Size	Average Number of Weeks Participated (a) Experimentals	Controls	Within-Subgroup Impact	p (b)	Between-Subgroups Impact Difference (c)	p (b)
Age (years)						--	0.228
16-17	408	28.0	13.7	14.3 ***	0.000		
18-19	996	23.2	13.1	10.1 ***	0.000		
20-22	682	27.7	15.1	12.6 ***	0.000		
Ethnicity						-- **	0.033
Black, non-Hispanic	1,093	26.1	14.3	11.8 ***	0.000		
Hispanic	465	29.1	13.4	15.7 ***	0.000		
White or other	527	21.8	13.5	8.2 ***	0.000		
Living arrangement						0.1	0.953
Living with mother	710	25.4	13.6	11.8 ***	0.000		
Not living with mother	1,352	25.8	14.1	11.6 ***	0.000		
Number of children						-3.6 *	0.080
1	1,356	25.7	15.1	10.5 ***	0.000		
More than 1	732	25.8	11.7	14.1 ***	0.000		
Age at first child's birth (years)						4.1 **	0.042
13-16	840	26.4	12.1	14.3 ***	0.000		
17-19	1,248	25.2	15.0	10.2 ***	0.000		
Age of youngest child (years)						0.1	0.943
Less than 1	1,119	25.6	13.7	11.8 ***	0.000		
1 or older	965	25.8	14.1	11.7 ***	0.000		
Educational attainment						13.4 ***	0.001
No high school diploma or GED	1,952	26.3	13.8	12.6 ***	0.000		
Had a high school diploma or GED	132	16.1	16.9	-0.8	0.847		

(continued)

TABLE 4.2 (continued)

Characteristic and Subgroup at Random Assignment	Sample Size	Average Number of Weeks Participated (a) Experimentals	Controls	Within-Subgroup Impact	p (b)	Between-Subgroups Impact Difference (c)	p (b)
Highest grade completed						6.1 ***	0.004
10th or below	1,384	26.2	12.4	13.8 ***	0.000		
11th or above	701	24.7	17.0	7.7 ***	0.000		
Interval since last attended regular high school						4.1 **	0.042
More than 2 years	1,097	25.5	11.7	13.8 ***	0.000		
2 years or less	934	26.2	16.5	9.7 ***	0.000		
TABE reading test score (grade equivalent) (d)						--	0.101
Below 6th grade	431	28.2	12.9	15.3 ***	0.000		
6th or 7th grade	482	25.7	11.6	14.1 ***	0.000		
8th or 9th grade	584	25.3	15.6	9.7 ***	0.000		
10th grade or above	585	24.2	14.5	9.7 ***	0.000		
Ever employed						-0.1	0.962
Yes	1,640	25.8	14.1	11.7 ***	0.000		
No	448	25.1	13.3	11.9 ***	0.000		
Prior-year earnings						0.8	0.747
$0-$500	1,660	25.8	13.9	11.9 ***	0.000		
$501 or more	418	25.0	13.9	11.1 ***	0.000		
Any AFDC received in household						-8.8 **	0.048
Yes	1,976	25.8	14.5	11.3 ***	0.000		
No	109	23.7	3.6	20.1 ***	0.000		
Family received AFDC when sample member was growing up						--	0.481
Always	344	25.1	13.0	12.1 ***	0.000		
Sometimes	977	24.8	14.3	10.6 ***	0.000		
Never	751	26.8	13.6	13.2 ***	0.000		

(continued)

TABLE 4.2 (continued)

Characteristic and Subgroup at Random Assignment	Sample Size	Average Number of Weeks Participated (a)		Within-Subgroup Impact		p (b)	Between-Subgroups Impact Difference (c)	p (b)
		Experimentals	Controls					
CES-D (depression) Scale (e)							--	0.630
0-15 (not at risk)	979	24.7	14.0	10.7	***	0.000		
16-23 (at some risk)	539	27.0	14.5	12.6	***	0.000		
24-60 (at high risk)	566	25.9	13.2	12.7	***	0.000		
Sample size	2,088							

SOURCES: MDRC calculations from New Chance Enrollment Form and survey data.

NOTES: Calculations for this table used data for all 2,088 sample members for whom there were 18 months of follow-up survey data, including those with values of zero for outcomes and New Chance enrollees (i.e., experimentals) who did not participate in the program. The reported sample sizes may fall short of this number because of missing or unusable items from some sample members' questionnaires.

The averages are adjusted using a two-way analysis of covariance procedure controlling for up to 51 kinds of difference in characteristics, other than the characteristic used to define subgroups, before random assignment. The two categories used as factors were research status (i.e., membership in the experimental or control group) and, one at a time, the baseline characteristics indicated. Rounding may cause slight discrepancies in sums and differences.

(a) For controls, services were obtained at programs or agencies other than New Chance. For experimentals, the services were obtained either at New Chance or, if they were served by additional programs, elsewhere.

(b) A two-tailed t-test was applied to each regression-adjusted within-subgroup impact and also, whenever there were two subgroups, to each difference between subgroup impacts. For each characteristic with more than two subgroups, an F-test was applied to the interaction between that characteristic and experimental or control status. The columns labeled "p" are the statistical significance levels of each within-subgroup impact and each between-subgroups difference in impacts: That is, p is the probability that sample estimates are different from zero or from each other only because of random error. Statistical significance levels are indicated as *** = 1 percent; ** = 5 percent; * = 10 percent.

(c) For each characteristic with only two subgroups, the between-subgroups impact difference is the impact for the first subgroup less the impact for the second subgroup. For characteristics with more than two subgroups, a between-subgroups impact difference cannot be calculated, as indicated by dashes in the table.

(d) The test used to measure reading ability was the reading part of the Tests of Adult Basic Education (TABE). Most sites administered the Survey Form of the test, but some administered the full reading test.

(e) The Center for Epidemiological Studies Depression (CES-D) Scale is a widely used measure of depression; scores can range from zero to 60.

had the greatest effect on length of stay in education activities for Hispanic young women, followed by young women who were black and then by whites and others. It had a greater effect on the educational attendance of enrollees who were 16 or younger when their first child was born than on those who were older. The program also had larger impacts on the following subgroups than on the other subgroups in their categories: those who, at baseline, had more than one child, had not received a high school diploma or GED, had completed less than tenth-grade, had been out of school more than two years, or were not receiving AFDC. The subgroup impact difference also approached statistical significance for the Tests of Adult Basic Education (TABE) reading score variable, suggesting that the impact on educational attendance was greatest for the poorest readers.

With the exception of the very small subgroup comprising those who were not on AFDC at random assignment, almost all of these subgroup impact differences point to the same general conclusion: New Chance was most effective in increasing the length of stay in education activities of those young women who were initially more disadvantaged. This is true in part because controls in the more disadvantaged subgroups remained in education activities for fewer weeks than controls who were more advantaged. But it is also the case that New Chance retained less as well as more advantaged young women in education activities for similar periods of time.

C. Impacts for Sites

There was considerable variation by site in the average length of time sample members spent in education programs. For experimentals, this ranged from 18.2 weeks in Chicago Heights to 32.0 weeks in Jacksonville, and for controls, from 8.7 weeks in Allentown to 22.6 weeks in Minneapolis. (See Table 4.3.)

At 13 of the 16 sites, program impacts favored the experimentals and were statistically significant, and at two of the remaining sites (Chicago Heights and Portland), the difference also favored the experimental group and approached statistical significance. In Minneapolis, controls reported attending education programs for more weeks than experimentals (22.6 weeks versus 19.5 weeks), but the difference was not statistically significant. The impact on length of stay in education programs was especially large for the Allentown, Jacksonville, Lexington, and Philadelphia sites.

III. Impacts on Participation in Employment-Related Activities

A. Aggregate Impacts

High levels of participation in education services were not matched by high levels of participation by either experimental or control group members in employment-related activities measured by the survey. (Vocational skills training, job search, and unpaid work experience were counted as employment-related, but not employability development activities.) As noted in Chapter 3, many New Chance enrollees left the program before Phase II, when the program focus shifted from attainment of a GED to direct preparation for employment.

Experimentals had higher rates of participation in all employment-related activities than did controls over the follow-up period, but the differences between the groups, while statistically significant, were not as large as might have been expected. As Table 4.4 shows, 33.3 percent of

TABLE 4.3

IMPACTS OF NEW CHANCE ON NUMBER OF WEEKS PARTICIPATED IN ANY EDUCATION PROGRAM WITHIN 18 MONTHS AFTER RANDOM ASSIGNMENT, BY SITE

Site	Sample Size	Average Number of Weeks Participated (a)		Within-Site Impact	p (b)	Between-Sites Impact Difference p (b)
		Experimentals	Controls			
						*** 0.003
Allentown	115	26.3	8.7	17.6 ***	0.000	
Bronx	126	18.5	11.7	6.8 *	0.092	
Chicago Heights	69	18.2	9.8	8.5	0.118	
Chula Vista	127	30.8	17.4	13.4 ***	0.001	
Denver	110	28.4	17.0	11.4 ***	0.007	
Detroit	169	22.1	9.6	12.6 ***	0.000	
Harlem	124	26.6	10.7	15.9 ***	0.000	
Inglewood	131	27.1	19.7	7.4 *	0.056	
Jacksonville	144	32.0	13.0	19.0 ***	0.000	
Lexington	135	31.7	12.7	19.1 ***	0.000	
Minneapolis	121	19.5	22.6	−3.1	0.448	
Philadelphia	135	29.7	11.5	18.3 ***	0.000	
Pittsburgh	171	20.1	9.4	10.7 ***	0.002	
Portland	143	20.1	14.3	5.8	0.116	
Salem	134	26.7	19.5	7.2 *	0.063	
San Jose	134	31.8	15.9	15.9 ***	0.000	
Sample size	2,088					

SOURCES: MDRC calculations from New Chance Enrollment Form and survey data.

NOTES: Calculations for this table used data for all 2,088 sample members for whom there were 18 months of follow-up survey data, including those with values of zero for outcomes and New Chance enrollees (i.e., experimentals) who did not participate in the program.

The averages are adjusted using a two-way analysis of covariance procedure controlling for up to 36 kinds of difference in characteristics, other than site, before random assignment. The two categories used as factors were research status (i.e., membership in the experimental or control group) and site. Rounding may cause slight discrepancies in sums and differences.

(a) For controls, services were obtained at programs or agencies other than New Chance. For experimentals, the services were obtained either at New Chance or, if they were served by additional programs, elsewhere.

(b) A two-tailed t-test was applied to each regression-adjusted within-site impact. An F-test was applied to the interaction between sites and experimental or control status. The columns showing p-values are the statistical significance levels of each within-site impact and each between-sites difference in impacts: That is, p is the probability that sample estimates are different from zero or from each other only because of random error. Statistical significance levels are indicated as *** = 1 percent; ** = 5 percent; * = 10 percent.

TABLE 4.4

IMPACTS OF NEW CHANCE ON PARTICIPATION IN EMPLOYMENT-RELATED ACTIVITIES WITHIN 18 MONTHS AFTER RANDOM ASSIGNMENT

Activity and Follow-Up Period (b)	Participated in Activity				Average Number of Weeks Participated				Average Scheduled Hours per Week for Those Who Participated in Activity (a)	
	Experimentals (%)	Controls (%)	Difference	p (c)	Experimentals	Controls	Difference	p (c)	Experimentals	Controls
Skills training or unpaid work experience										
Quarters 1-6 (d)	35.2	23.3	11.8 ***	0.000	8.2	5.4	2.8 ***	0.000	N/A (e)	N/A
Quarters 1-4	27.1	16.0	11.1 ***	0.000	5.9	3.0	2.9 ***	0.000	N/A	N/A
Quarters 5-6	19.2	15.2	4.0 **	0.024	2.3	2.3	0.0	0.891	N/A	N/A
Attending skills training or unpaid work experience at follow-up	6.8	6.0	0.9	0.449	-- (f)	--	--	--	--	--
Skills training										
Quarters 1-6	33.3	22.5	10.8 ***	0.000	7.6	5.1	2.5 ***	0.000	20.1	24.7
Quarters 1-4	25.8	15.3	10.5 ***	0.000	5.5	3.0	2.6 ***	0.000	18.9	24.3
Quarters 5-6	18.1	14.6	3.5 **	0.041	2.1	2.2	0.0	0.872	23.2	24.2
Attending skills training at follow-up	6.8	6.0	0.9	0.449	--	--	--	--	--	--
Unpaid work experience										
Quarters 1-6	6.3	2.2	4.1 ***	0.000	0.6	0.2	0.3 **	0.041	N/A (e)	N/A
Quarters 1-4	3.8	1.2	2.6 ***	0.001	0.4	0.1	0.3 ***	0.007	N/A	N/A
Quarters 5-6	3.0	1.5	1.5 **	0.034	0.2	0.2	0.0	0.955	N/A	N/A
Attending unpaid work experience at follow-up	0.0	0.0	0.0	1.000	--	--	--	--	--	--
Job search classes (g)										
Quarters 1-6	28.4	14.5	13.9 ***	0.000	6.2	1.7	4.5 ***	0.000	8.3	11.2
Quarters 1-4	24.6	10.2	14.4 ***	0.000	5.3	1.2	4.1 ***	0.000	8.1	10.5
Quarters 5-6	9.7	6.2	3.5 ***	0.008	1.0	0.6	0.4 **	0.019	8.9	10.7
Attending job search classes at follow-up	2.4	1.3	1.1	0.111	--	--	--	--	--	--
Sample size	1,408	680			1,408	680				

(continued)

TABLE 4.4 (continued)

SOURCES: MDRC calculations from New Chance Enrollment Form and survey data.

NOTES: Calculations for this table used data for all 2,088 sample members for whom there were 18 months of follow-up survey data, including those with values of zero for outcomes and New Chance enrollees (i.e., experimentals) who did not participate in the program.

The averages or percentages are adjusted using linear analysis of covariance procedures controlling for up to 51 kinds of difference in characteristics before random assignment. Rounding may cause slight discrepancies in sums and differences.

(a) These columns contain data only for those who participated in a given activity rather than for the full research sample.

(b) For controls, services were obtained at or arranged through programs or agencies other than New Chance. For experimentals, the services were obtained at or arranged through New Chance or, if they were served by additional programs, by these programs.

(c) A two-tailed t-test was applied to each regression-adjusted difference between average experimental and control group outcomes. The column labeled "p" is the statistical significance level of the difference between experimental and control group outcomes: That is, p is the probability that average outcomes are different only because of random error. Statistical significance levels are indicated as *** = 1 percent; ** = 5 percent; * = 10 percent.

(d) Quarter 1 refers to the three calendar months beginning with the month in which the sample member was randomly assigned to the experimental or control group. Thus, e.g., for a young woman who was randomly assigned on May 16, 1990, quarter 1 means the period from May 1 through July 31, 1990.

(e) Data were not collected on the number of hours for which work experience was scheduled, as indicated by the symbol N/A (not available).

(f) Where data are not applicable, dashes are used.

(g) These were classes or assistance sometimes called "job club" or "job search" lasting for a few weeks in preparing resumes and job applications, or calling employers.

experimentals and 22.5 percent of controls participated in skills training during the follow-up period; on average, experimentals participated in skills training for 7.6 weeks and controls for 5.1 weeks, also a significant difference.[9] (However, the length of stay in skills training for those who actually participated in it was virtually identical for both groups: 22.8 weeks for experimentals and 22.7 weeks for controls.) It appears that experimentals were more likely to enter office skills training courses, while controls were more apt to train as nurse's aides or practical nurses (not shown in tables), but because many young women did not name a specific occupation for which they were training, this conclusion must be regarded as tentative. Completion of skills training and receipt of a training certificate are considered in the next chapter.

Few members of either group (6.3 percent of experimentals and 2.2 percent of controls) said that they had held an unpaid job intended to give them experience working, but the difference between the groups was statistically significant.[10] Experimentals were also significantly more likely than controls (28.4 percent versus 14.5 percent) to report having participated in formal job search or job club activities – i.e., classes lasting for a few weeks that included time spent both in learning how to prepare resumes and fill out job applications, and in actually calling employers. In some cases, these classes were delivered by the New Chance program operator; in others, they were run by the local welfare department.

As was true of participation in education activities, differences between the groups in rates of participation in employment-related activities narrowed over time. At the follow-up interview, only a small proportion of the members of either group were participating in any of these activities, and differences between the groups were no longer statistically significant.

B. Impacts for Subgroups

Table 4.5 is analogous in format to Table 4.2. Across most, but not all, of the subgroups that were examined, experimentals reported significantly more weeks of participation in skills training than did controls. However, in contrast to the impacts on time in education activities, which were larger for the *more* disadvantaged subgroups, impacts on time in skills training were especially notable for young women in the experimental group who were *less* disadvantaged when they entered New Chance. Thus, impacts were more pronounced for women who were 17 to 19 years old, rather than younger, when they first gave birth. They were also larger for those who had a high school diploma or GED when they entered the sample. The latter difference is probably accounted for the fact that, as noted in Chapter 3, the New Chance sites tended to move high school graduates into training programs

---

[9] Because of the sequential arrangement of education and skills training in New Chance, more experimentals than controls had a GED in hand when they entered skills training. Half of the young women in the experimental group who entered skills training had earned a GED within the follow-up period; half had not. Many of the latter probably entered skills training independent of New Chance, after they had left the program. Controls were less likely than experimentals to have entered skills training in the first place. Of those who did, however, only a third earned a GED during the follow-up period. On their own or with outside assistance, controls evidently located training programs that were willing to accept students who did not already have this credential.

[10] Paid employment is considered in Chapter 8.

TABLE 4.5

IMPACTS OF NEW CHANCE ON NUMBER OF WEEKS PARTICIPATED IN SKILLS TRAINING WITHIN 18 MONTHS AFTER RANDOM ASSIGNMENT, BY SUBGROUP

Characteristic and Subgroup at Random Assignment	Sample Size	Average Number of Weeks Participated (a)		Within-Subgroup Impact	p (b)	Between-Subgroups Impact Difference (c)	p (b)
		Experimentals	Controls				
Age (years)						--	0.281
16-17	408	9.1	8.3	0.9	0.575		
18-19	996	8.1	4.6	3.5 ***	0.000		
20-22	682	6.1	4.2	1.9 *	0.093		
Ethnicity						--	0.105
Black, non-Hispanic	1,093	7.1	5.8	1.3	0.164		
Hispanic	465	9.7	5.2	4.5 ***	0.001		
White or other	527	7.0	3.7	3.4 ***	0.010		
Living arrangement						-0.1	0.919
Living with mother	710	6.9	4.5	2.4 **	0.035		
Not living with mother	1,352	7.9	5.4	2.5 ***	0.002		
Number of children						1.8	0.179
1	1,356	7.9	4.7	3.2 ***	0.000		
More than 1	732	7.1	5.8	1.3	0.221		
Age at first child's birth (years)						-3.0 **	0.025
13-16	840	7.1	6.4	0.7	0.498		
17-19	1,248	8.0	4.3	3.7 ***	0.000		
Age of youngest child (years)						1.6	0.222
Less than 1	1,119	8.1	4.8	3.3 ***	0.000		
1 or older	965	7.2	5.5	1.7 *	0.088		
Educational attainment						-6.1 **	0.027
No high school diploma or GED	1,952	6.9	4.8	2.1 ***	0.002		
Had a high school diploma or GED	132	18.0	9.8	8.2 ***	0.002		

(continued)

TABLE 4.5 (continued)

Characteristic and Subgroup at Random Assignment	Sample Size	Average Number of Weeks Participated (a)		Within-Subgroup Impact	p (b)	Between-Subgroups Impact Difference (c)	p (b)
		Experimentals	Controls				
Highest grade completed						0.1	0.966
10th or below	1,384	7.1	4.6	2.5 ***	0.002		
11th or above	701	8.7	6.3	2.5 **	0.031		
Interval since last attended regular high school						1.0	0.441
More than 2 years	1,097	7.1	3.9	3.1 ***	0.001		
2 years or less	934	8.5	6.4	2.1 **	0.036		
TABE reading test score (grade equivalent) (d)						--	0.830
Below 6th grade	431	5.7	4.4	1.3	0.377		
6th or 7th grade	482	8.3	5.7	2.6 *	0.062		
8th or 9th grade	584	7.6	4.8	2.9 **	0.020		
10th grade or above	585	8.5	5.6	2.9 **	0.018		
Ever employed						1.7	0.286
Yes	1,640	8.0	5.2	2.9 ***	0.000		
No	448	6.2	5.0	1.2	0.395		
Prior-year earnings						-2.1	0.208
$0-$500	1,660	7.3	5.2	2.1 ***	0.005		
$501 or more	418	9.0	4.8	4.2 ***	0.005		
Any AFDC received in household						0.4	0.886
Yes	1,976	7.6	5.0	2.6 ***	0.000		
No	109	9.3	7.2	2.1	0.464		
Family received AFDC when sample member was growing up						--	0.325
Always	344	7.3	2.5	4.8 ***	0.004		
Sometimes	977	7.2	5.3	2.0 **	0.037		
Never	751	8.4	6.2	2.3 **	0.040		

(continued)

TABLE 4.5 (continued)

Characteristic and Subgroup at Random Assignment	Sample Size	Average Number of Weeks Participated (a)		Within-Subgroup Impact	p (b)	Between-Subgroups Impact Difference (c)	p (b)
		Experimentals	Controls				
CES-D (depression) Scale (e)						--	0.444
0-15 (not at risk)	979	7.9	4.6	3.3 ***	0.001		
16-23 (at some risk)	539	7.5	5.1	2.5 *	0.053		
24-60 (at high risk)	566	7.4	6.1	1.3	0.318		
Sample size	2,088						

SOURCES: MDRC calculations from New Chance Enrollment Form and survey data.

NOTES: Calculations for this table used data for all 2,088 sample members for whom there were 18 months of follow-up survey data, including those with values of zero for outcomes and New Chance enrollees (i.e., experimentals) who did not participate in the program. The reported sample sizes may fall short of this number because of missing or unusable items from some sample members' questionnaires.

The averages are adjusted using a two-way analysis of covariance procedure controlling for up to 51 kinds of difference in characteristics, other than the characteristic used to define subgroups, before random assignment. The two categories used as factors were research status (i.e., membership in the experimental or control group) and, one at a time, the baseline characteristics indicated. Rounding may cause slight discrepancies in sums and differences.

(a) For controls, services were obtained at or arranged through programs or agencies other than New Chance. For experimentals, the services were obtained at or arranged through New Chance or, if they were served by additional programs, by these programs.

(b) A two-tailed t-test was applied to each regression-adjusted within-subgroup impact and also, whenever there were two subgroups, to each difference between subgroup impacts. For each characteristic with more than two subgroups, an F-test was applied to the interaction between that characteristic and experimental or control status. The columns labeled "p" are the statistical significance levels of each within-subgroup impact and each between-subgroups difference in impacts: That is, p is the probability that sample estimates are different from zero or from each other only because of random error. Statistical significance levels are indicated as *** = 1 percent; ** = 5 percent; * = 10 percent.

(c) For each characteristic with only two subgroups, the between-subgroups impact difference is the impact for the first subgroup less the impact for the second subgroup. For characteristics with more than two subgroups, a between-subgroups impact difference cannot be calculated, as indicated by dashes in the table.

(d) The test used to measure reading ability was the reading part of the Tests of Adult Basic Education (TABE). Most sites administered the Survey Form of the test, but some administered the full reading test.

(e) The Center for Epidemiological Studies Depression (CES-D) Scale is a widely used measure of depression; scores can range from zero to 60.

within a couple of months after enrollment, whereas training for other enrollees was deferred until they had earned a GED.

C. Impacts for Sites

Table 4.6 indicates that the impact of New Chance on time spent in skills training differed markedly among the 16 sites. Only at the Portland site, where education and skills training were delivered concurrently and most experimentals participated in both, was the program effect positive, statistically significant, and sizeable (25 weeks on average for experimentals versus 5.4 weeks for controls). At 11 of the remaining 15 sites, the difference favored the experimentals, but in none of the cases was it large enough to be statistically significant.

IV. Impacts on Participation in Parenting and Personal Development Activities

As Table 4.7 makes clear, experimentals were far more likely than controls to participate in parenting classes and to receive other services aimed at their personal development: classes on family planning, health, and life skills, or personal and job counseling. While the majority of controls participated in education activities of some kind during the 18-month follow-up period, as shown in Table 4.1, a relatively small proportion (one-fifth or fewer) reported having received parenting or personal development services, compared to about half of the experimentals.

Experimentals were not only more likely to participate at all in these activities, but they also received a significantly larger "dose" of all services. For example, four times as many experimentals as controls reported attending parenting and life skills classes 11 or more times during the follow-up period; disparities between the groups in the reported receipt of health education and family planning classes were even sharper.

V. Sample Members' Ratings of the Services They Received

As a measure of their satisfaction with the services they had obtained, both experimentals and controls who received a specific service were asked to indicate how much they would recommend that service to a friend, using a scale of 0 to 10 (with 10 being the highest possible recommendation). Table 4.8 shows the survey respondents' average ratings of 12 different services. The ratings are descriptive only; impacts cannot be derived from them because only those sample members who got the service were asked the question.

The table reveals that members of both groups tended to recommend the services they received quite favorably: All but one service received a rating of 7 or higher, and the ratings for the different kinds of services were similar. (The exception was a rating of 5.6 given to high school classes by the very small percentage of controls who attended them.) Experimentals' ratings of services tended to be higher than those of controls, but rarely by more than half a point.

TABLE 4.6

IMPACTS OF NEW CHANCE ON NUMBER OF WEEKS PARTICIPATED IN SKILLS TRAINING WITHIN 18 MONTHS AFTER RANDOM ASSIGNMENT, BY SITE

Site	Sample Size	Average Number of Weeks Participated (a)		Within-Site Impact		p (b)	Between-Sites Impact Difference p (b)
		Experimentals	Controls				
							*** 0.000
Allentown	115	7.3	3.8	3.5		0.202	
Bronx	126	9.2	6.1	3.1		0.241	
Chicago Heights	69	4.9	1.6	3.3		0.353	
Chula Vista	127	7.3	7.1	0.2		0.948	
Denver	110	10.4	9.6	0.8		0.788	
Detroit	169	6.5	9.4	−2.9		0.201	
Harlem	124	4.0	2.0	2.0		0.454	
Inglewood	131	9.1	7.6	1.5		0.557	
Jacksonville	144	5.1	2.7	2.5		0.319	
Lexington	135	3.5	4.2	−0.8		0.772	
Minneapolis	121	2.0	4.3	−2.3		0.393	
Philadelphia	135	10.2	7.3	2.8		0.265	
Pittsburgh	171	6.9	3.6	3.3		0.138	
Portland	143	25.0	5.4	19.6	***	0.000	
Salem	134	4.8	1.2	3.6		0.160	
San Jose	134	4.1	4.8	−0.7		0.791	
Sample size	2,088						

SOURCES: MDRC calculations from New Chance Enrollment Form and survey data.

NOTES: Calculations for this table used data for all 2,088 sample members for whom there were 18 months of follow-up survey data, including those with values of zero for outcomes and New Chance enrollees (i.e., experimentals) who did not participate in the program.

The averages are adjusted using a two-way analysis of covariance procedure controlling for up to 36 kinds of difference in characteristics, other than site, before random assignment. The two categories used as factors were research status (i.e., membership in the experimental or control group) and site. Rounding may cause slight discrepancies in sums and differences.

(a) For controls, services were obtained at or arranged through programs or agencies other than New Chance. For experimentals, the services were obtained at or arranged through New Chance or, if they were served by additional programs, by these programs.

(b) A two-tailed t-test was applied to each regression-adjusted within-site impact. An F-test was applied to the interaction between sites and experimental or control status. The columns showing p-values are the statistical significance levels of each within-site impact and each between-sites difference in impacts: That is, p is the probability that sample estimates are different from zero or from each other only because of random error. Statistical significance levels are indicated as *** = 1 percent; ** = 5 percent; * = 10 percent.

TABLE 4.7

IMPACTS OF NEW CHANCE ON PARTICIPATION IN CLASSES ON PARENTING, FAMILY PLANNING, HEALTH, AND LIFE SKILLS WITHIN 18 MONTHS AFTER RANDOM ASSIGNMENT

Activity and Frequency of Attendance/Receipt (a)	Experimentals (%)	Controls (%)	Difference	p (b)
Parenting classes			***	0.000
Ever attended	66.5	20.6	45.9	
10 times or fewer	26.5	10.8	15.7	
11 times or more	40.0	9.7	30.2	
Never attended	33.5	79.4	−45.9	
Family planning classes			***	0.000
Ever attended	51.7	11.9	39.8	
10 times or fewer	29.4	9.5	19.9	
11 times or more	22.4	2.4	19.9	
Never attended	48.3	88.1	−39.8	
Health classes			***	0.000
Ever attended	49.3	11.0	38.3	
10 times or fewer	25.1	8.4	16.7	
11 times or more	24.2	2.6	21.6	
Never attended	50.7	89.0	−38.3	
Personal counseling			***	0.000
Ever attended	40.9	14.6	26.4	
10 times or fewer	23.2	8.6	14.6	
11 times or more	17.7	5.9	11.8	
Never attended	59.1	85.4	−26.3	
Job counseling			***	0.000
Ever attended	53.6	19.4	34.2	
10 times or fewer	27.2	10.5	16.7	
11 times or more	26.4	8.9	17.6	
Never attended	46.4	80.6	−34.2	
Life skills classes			***	0.000
Ever attended	51.6	12.4	39.2	
10 times or fewer	23.0	6.0	16.9	
11 times or more	28.6	6.4	22.3	
Never attended	48.4	87.6	−39.2	
Sample size	1,408	680		

(continued)

TABLE 4.7 (continued)

SOURCES: MDRC calculations from New Chance Enrollment Form and survey data.

NOTES: Calculations for this table used data for all 2,088 sample members for whom there were 18 months of follow-up survey data, including those with values of zero for outcomes and New Chance enrollees (i.e., experimentals) who did not participate in the program.

The average outcomes are mean predicted probabilities from multinomial logit procedures controlling for up to 51 kinds of difference in characteristics before random assignment. Rounding may cause slight discrepancies in sums and differences.

(a) For controls, services were obtained at programs or agencies other than New Chance. For experimentals, the services were obtained either at New Chance or, if they were served by additional programs, elsewhere.

(b) Likelihood-ratio chi-square tests of statistical significance were conducted for each panel. The column labeled "p" is the statistical significance level of the difference between experimental and control group outcomes: That is, p is the probability that average outcomes are different only because of random error. Statistical significance levels are indicated as *** = 1 percent; ** = 5 percent; * = 10 percent.

TABLE 4.8

HOW EXPERIMENTALS AND CONTROLS RATED VARIOUS ACTIVITIES IN WHICH THEY PARTICIPATED

	Experimentals (a)		Controls (b)	
Activity (c)	Sample Size	Average Rating	Sample Size	Average Rating
High school	18	8.2	8	5.6
Basic education/GED	578	7.9	159	7.6
College classes	100	8.1	26	8.4
Other education classes	58	7.9	36	7.8
Skills training	254	8.1	82	7.1
Job club	220	8.2	44	8.1
Parenting classes	932	8.2	138	8.2
Family planning classes	724	8.7	81	8.1
Health classes	690	8.4	74	7.6
Personal counseling	573	8.3	97	8.3
Job counseling	749	8.3	131	8.0
Life skills classes	723	8.5	84	8.6

SOURCE: MDRC calculations from survey data.

NOTES: Sample members were asked if they attended the activity, and if so, how much they would recommend that a friend also attend it. Ratings were on an 11−point scale, where 0 meant "not at all" and 10 meant "the most possible."

(a) Sample sizes vary because different numbers of sample members participated in different classes or activities.

(b) These columns contain data only for those who participated in a given activity rather than for the full research sample.

(c) For controls, services were obtained at or arranged through programs or agencies other than New Chance. For experimentals, the services were obtained at or arranged through New Chance or, if they were served by additional programs, by these programs.

VI. New Chance and Other Programs Compared

Table 4.9 compares service receipt by experimentals and controls in New Chance and in the four other programs briefly described in Chapter 1: the JOBSTART Demonstration, the LEAP program, the Teenage Parent Demonstration, and Project Redirection.[11] The large number of cells in the table for which data were not available give evidence of the problems involved in drawing such comparisons – problems that include differences in the variables examined (e.g., participation in education versus participation in education or training), in the follow-up periods adopted (e.g., 18 months versus 24 months), in the mandatory (LEAP and the Teenage Parent Demonstration) or voluntary (New Chance, essentially, as well as JOBSTART and Project Redirection) nature of the programs, and in the populations served. With regard to the last point, JOBSTART (which targeted youths of both sexes, and young women who did and did not have children) and New Chance were targeted primarily toward high school dropouts; LEAP, the Teenage Parent Demonstration, and Project Redirection all enrolled young women who were attending school at baseline, as well as those who had dropped out. To give greater validity to the comparisons, the statistics for LEAP and the Teenage Parent Demonstration pertain only to the subgroup of sample members in each demonstration who were out of school and did not have a high school diploma or GED at baseline. (Even so, the populations served in these two programs were, on average, somewhat younger than the New Chance population.) The Project Redirection results are for the full enrollee population, including those who were enrolled in school at baseline, because data limited to the dropout subgroup were not consistently available.

The table indicates that, not surprisingly, substantially higher percentages of experimentals and controls participated in education and skills training in the three voluntary programs (New Chance, JOBSTART, and Project Redirection) than in the two mandatory ones (LEAP and the Teenage Parent Demonstration). This result reflects the fact that the young mothers in the voluntary programs were not typical of all young mothers who are high school dropouts; rather, they enrolled in the programs especially to receive these services.

Similar proportions of experimentals in New Chance and in JOBSTART participated in education or skills training. But the percentage of New Chance *controls* who received these services was much higher than the corresponding proportion of controls in JOBSTART. (Indeed, New Chance controls received more services than did the experimentals in the mandatory programs.) This fact must be borne in mind when the impacts of New Chance and JOBSTART are compared as to educational attainment (Chapter 5) and employment (Chapter 8).

---

[11] All of these operated as demonstration programs except LEAP, which is a statewide program in Ohio.

TABLE 4.9

A COMPARISON OF PROGRAM IMPACTS ON SERVICE RECEIPT IN NEW CHANCE AND OTHER SELECTED PROGRAMS FOR YOUNG MOTHERS

	New Chance			JOBSTART Mothers (b)			LEAP Dropout Subsample (c)		
Service and Follow-Up Period	Experimentals (%)	Controls (%)	Difference	Experimentals (%)	Controls (%)	Difference	Experimentals (%)	Controls (%)	Difference
Participated in any education program									
Quarters 1-4	81.7	48.4	33.3 ***	N/A	N/A	N/A	46.8	33.4	13.4 ***
Quarters 1-8	85.3	60.4	24.9 *** (a)	N/A	N/A	N/A	N/A	N/A	N/A
Participated in any skills training program									
Quarters 1-4	25.8	15.3	10.5 ***	N/A	N/A	N/A	N/A	N/A	N/A
Quarters 1-8	33.3	22.5	10.8 *** (a)	N/A	N/A	N/A	N/A	N/A	N/A
Participated in any education or skills training program									
Quarters 1-4	87.0	55.3	31.7 ***	91.1	29.2	61.9 ***	N/A	N/A	N/A
Quarters 1-8	90.7	69.0	21.6 *** (a)	93.6	47.9	45.7 **	N/A	N/A	N/A
Received any parenting education									
Quarters 1-4	N/A	N/A	N/A	N/A	N/A	N/A	N/A	N/A	N/A
Quarters 1-6	66.5	20.6	45.9 ***	N/A	N/A	N/A	N/A	N/A	N/A
Received any family planning counseling									
Quarters 1-4	N/A	N/A	N/A	N/A	N/A	N/A	N/A	N/A	N/A
Quarters 1-6	51.7	11.9	39.8 ***	N/A	N/A	N/A	N/A	N/A	N/A
Sample size	1,408	680		250	234		256	264	

(continued)

TABLE 4.9 (continued)

Service and Follow-Up Period	Teenage Parent Demonstration Dropout Subsample (d) Experimentals (%)	Controls (%)	Difference	Project Redirection (e) Experimentals (%)	Comparison Group (%)	Difference
Participated in any education program						
Quarters 1–4	N/A	N/A	N/A	75.0	51.0	24.0 ***
Quarters 1–8	42.0	21.6	20.4 **	87.0	71.0	16.0 ***
Participated in any skills training program						
Quarters 1–4	N/A	N/A	N/A	N/A	N/A	N/A
Quarters 1–8	16.3	16.4	–0.1	N/A	N/A	N/A
Participated in any education or skills training program						
Quarters 1–4	N/A	N/A	N/A	N/A	N/A	N/A
Quarters 1–8	N/A	N/A	N/A	N/A	N/A	N/A
Received any parenting education						
Quarters 1–4	N/A	N/A	N/A	64.0	40.0	24.0 ***
Quarters 1–6	N/A	N/A	N/A	N/A	N/A	N/A
Received any family planning counseling						
Quarters 1–4	N/A	N/A	N/A	74.0	63.0	11.0 ***
Quarters 1–6	N/A	N/A	N/A	N/A	N/A	N/A
Sample size		1,162 (f)		305	370	

(continued)

TABLE 4.9 (continued)

SOURCES: MDRC calculations from New Chance Enrollment Form and survey data; Bloom, Fellerath et al., 1993; Cave and Doolittle, 1991; Maynard, Nicholson, and Rangarajan, 1993; Polit and White, 1988; Quint and Riccio, 1985.

NOTES: N/A indicates that the specified data item was not available.

A two-tailed t-test was applied to regression-adjusted differences between the experimental and control groups. Statistical significance levels are indicated as *** = 1 percent; ** = 5 percent; * = 10 percent.

(a) Data are for quarters 1 through 6 only.

(b) The JOBSTART sample is made up of young mothers between the ages of 17 and 21 who did not have a high school diploma or GED, read below the eighth-grade level, lived with their own children, and were not enrolled in school at the time of sample enrollment.

(c) The LEAP dropout subsample is made up of teenage custodial parents (almost all of whom were women) aged 19 or younger who were on welfare, did not have a high school diploma or GED, and were not enrolled in school at the time of sample enrollment.

(d) The Teenage Parent Demonstration dropout subsample is made up of custodial mothers on welfare who had only one child, were between the ages of 16 and 19, did not have a high school diploma or GED, and were not enrolled in school at the time of sample enrollment.

(e) The Project Redirection sample is made up of young mothers aged 17 or younger, most of whom were on welfare. This sample includes both women who were enrolled in school and those who had dropped out. In Project Redirection, unlike other demonstrations, the comparison group was not selected through random assignment.

(f) The sample is split roughly evenly between the experimental and control groups. Breakdowns by service category are not available.

CHAPTER 5

IMPACTS ON EDUCATIONAL ATTAINMENT AND ACHIEVEMENT

I. Introduction

Improving participants' educational status was perhaps the single most important short-term goal of New Chance because it was central to the program's vision of how participants could best achieve long-term well-being, economic and otherwise.

A. The Relationship Between Educational Attainment and Other Outcomes

A large body of literature documents the strong correlation between education and positive labor market outcomes: increased rates of employment, better-quality jobs, and higher incomes (Becker, 1974; Mincer, 1974; Levy and Michel, 1988; Blackburn, Bloom, and Freeman, 1989; Murphy and Welch, 1989; Danziger, 1991). Employers assess prospective employees partly on the basis of their acquisition of a high school diploma, a college degree, or other education credentials – i.e., their educational "attainment." These credentials, in turn, are commonly accepted tokens of both educational "achievement" – i.e., the mastery of academic skills (at the most basic level, literacy and numeracy) needed to perform work tasks – and good work habits such as patience and persistence (see Berg, 1969).

Prior research also supports a strong positive association between parental educational levels and the social and cognitive development of their children (D'Amico, Haurin, and Mott, 1983; Desai, Michael, and Chase-Lansdale, 1990). Most of this research, however, is correlational and cross-sectional: It looks at the associations between education and other outcomes for groups of individuals at a particular point in time. Much less is known about the effects of changing the educational level of a specific group of individuals.

Recently, the value of the GED in the labor market has become a subject of much policy debate. Some scholars have found the value of this credential to be limited, especially when compared to the labor market value of a regular high school diploma.[1] It is argued that preparation for the GED test is generally too short to add significantly to test-takers' cognitive skills. The sponsors of the GED test themselves report that, although the pass rate is about 70 percent, the average examinee spends only about 30 hours studying for the test; reportedly, many GED recipients pass it with no preparation whatsoever. Murnane and Willett (1993) express concern about the GED's potential perverse effect on high school attrition. They argue that the availability of the GED may induce some high school students to drop out, in which case obtaining the GED credential is meager compensation for lost time in school. Many institutions, most notably the U.S. Army, do not accept the GED as an alternative to a regular high school diploma. To institutions such as the Army, attainment of the GED has come to signify a lack of perseverance and discipline, a conclusion based on past experiences with GED attainers.

---

[1] See Cameron and Heckman, 1993. Among a sample of 25-year-old men, dropouts earned $10,379, GED recipients earned $11,777, and high school graduates earned $15,214 annually. The weak earnings advantage of GED recipients over dropouts is attributed to the fact that GED recipients had an extra year of regular high school, rather than to their receipt of a GED per se.

On the other hand, Murnane and Willett (1993) and Maloney (1991) have found that the GED often appears to fulfill a "gatekeeper" function for subsequent education and training. That is, even if the credential does not appear to have much of a direct labor market impact, it is frequently a prerequisite to entry into skills training, and that post-GED training is expected to increase long-term labor market prospects for those GED attainers who use the credential to pursue it.

Also, most of the analyses informing the discussion of the GED's value are based on the National Longitudinal Survey of Youth (NLSY), which follows a national sample of youths over time. Cave and Bos (1994) point out that individuals in this sample differ in many important ways from youths who volunteer for "second chance" education programs such as New Chance. While, for many NLSY youths, the GED may mean little more than a successful test score, for those participating in programs such as New Chance, the credential may signify achievement of a program milestone leading to jobs and opportunities for further training. In a nonexperimental analysis of data from the JOBSTART Demonstration, which also involved a population of high school dropouts who volunteered for remedial education and skills training, Cave and Bos found significant increases in subsequent earnings owing to program-induced GED attainment.

New Chance rested in large part on two central hypotheses. The first was that program participation would result in better education outcomes for young welfare mothers, in terms of both educational attainment and educational achievement. In this regard, because program enrollees had typically been out of school for so long and because it would require an unduly long time for them to earn a regular high school diploma, preparing students to take and pass the GED test was the primary goal of New Chance education classes.[2] Raising reading scores by two grade levels was an alternative goal for those whose reading scores at program entry suggested that a GED would be unattainable within the program time frame. The second hypothesis was that, in conjunction with gains realized from participation in other program services (such as occupational skills training and parenting instruction), these improved education outcomes would, over time, result in improved labor market outcomes for the young mothers and contribute to more favorable developmental outcomes for their children. "Over time" was an important condition of the formulation: As discussed in Chapter 8, "investing" in education may mean forgoing immediate opportunities to work in the interest of longer-term economic gains. This chapter examines the first hypothesis; Chapter 8 presents early evidence regarding the second.

B. The Educational Status of the New Chance Sample at Random Assignment

Data collected at baseline indicated that the New Chance sites faced sizeable challenges in improving participants' educational standing. Sample members had high educational aspirations: Almost equal proportions said that they wanted to complete high school, to attend some college, and to receive a college or graduate degree.[3] But, as a group, they faced many education-related barriers, although there was also considerable variation among individuals. With respect to educational attainment, only a handful (6.3 percent) of young mothers entered the research with a high school diploma or GED in hand. The average sample member had dropped out toward the end of the tenth grade, and over a third had completed only the ninth grade or less. Reading levels, a measure of educational achievement, varied greatly. While the sample average indicated an ability to read at the 8.4-grade level, 30 percent of sample members read at the tenth-grade level or higher, and one in seven (14.3 percent) read at the fifth-grade level or lower (see Table 2.1).

---

[2] After the period covered by this report, the Lexington program added a high school component for young women for whom earning a high school diploma appeared to be a reasonable prospect.

The statistics also indicated the prevalence of a number of other characteristics that might have been expected to impede the young women's educational progress. On average, sample members had been out of school for just under two and a half years when they entered the research sample.[4] A third (33.5 percent) had left school three or more years earlier, while just under a quarter (23.2 percent) had left school less than one year before sample entry. Two in five sample members had repeated a grade, a signal of their failure to have mastered academic material at the same rate as their peers, and 37.2 percent reported that they had dropped out of school before their first pregnancy, perhaps a token of their disaffection from the educational process.

C. A Preview of the Findings

This chapter first considers program effects on educational attainment (with a special focus on factors associated with GED receipt) and then examines educational achievement. The findings confirm that the greater participation of experimentals in education and training activities discussed in Chapter 4 paid off in terms of significant and positive program impacts on receipt of a GED and of college credits, but not on receipt of a high school diploma or a trade certificate or license. These impacts of New Chance on educational attainment were not, however, matched by impacts on educational achievement. Reading proficiency, as measured by a standardized test, was identical for both groups at the 18-month point, and, on average, the young women read at only the 7.8-grade level at follow-up.

II. Impacts on the Attainment of Education Credentials

A. Aggregate Impacts

Table 5.1 indicates that, at the 18-month follow-up, experimentals were significantly more likely than controls to have earned a high school diploma or a GED: 43.1 percent of the experimentals versus 30.0 percent of the controls had earned one of these credentials. Experimentals were actually less likely than controls to have earned a high school diploma (6.6 percent versus 9.2 percent, a difference that, while small was statistically significant), but more likely to have received a GED (36.8 percent versus 21.1 percent).

---

[3] Sample members had even higher levels of expectation for their children: 79 percent said that they expected their child to complete education beyond the secondary school level.

[4] It is likely that many sample members attended school only sporadically before formally dropping out.

TABLE 5.1

IMPACTS OF NEW CHANCE ON EDUCATIONAL ATTAINMENT AT OR WITHIN 12 AND 18 MONTHS AFTER RANDOM ASSIGNMENT

Outcome and Follow-Up Period	Experimentals (%)	Controls (%)	Difference		p (a)
Education credential at 18 months (b)					
GED or high school diploma	43.1	30.0	13.1	***	0.000
GED	36.8	21.1	15.8	***	0.000
High school diploma	6.6	9.2	−2.6	***	0.004
Trade certificate or license	12.5	12.4	0.1		0.931
Credits toward A.A. or B.A. degree	9.8	7.1	2.6	**	0.041
Education credential at 12 months (b)					
GED or high school diploma	35.2	23.0	12.3	***	0.000
GED	29.7	14.8	15.0	***	0.000
High school diploma	5.7	8.3	−2.6	***	0.002
Trade certificate or license	7.6	8.6	−1.0		0.427
Credits toward A.A. or B.A. degree	0.0	0.0	0.0		1.000
Sample size	1,408	680			

SOURCES: MDRC calculations from New Chance Enrollment Form and survey data.

NOTES: Calculations for this table used data for all 2,088 sample members for whom there were 18 months of follow-up survey data, including those with values of zero for outcomes and New Chance enrollees (i.e., experimentals) who did not participate in the program.

The percentages are adjusted using linear analysis of covariance procedures controlling for up to 51 kinds of difference in characteristics before random assignment. Rounding may cause slight discrepancies in sums and differences.

(a) A two-tailed t-test was applied to each regression-adjusted difference between average experimental and control group outcomes. The column labeled "p" is the statistical significance level of the difference between experimental and control group outcomes: That is, p is the probability that average outcomes are different only because of random error. Statistical significance levels are indicated as *** = 1 percent; ** = 5 percent; * = 10 percent.

(b) The percentages shown are for all sample members, including the 6 percent who had already achieved a high school diploma or GED when they applied to the program.

While the majority of young women who earned a GED did so during the first year after random assignment, a sizeable minority of young women in both research groups who received this certificate (19 percent of the experimentals who earned a GED and 30 percent of the controls) did so during the last six months of the follow-up period. New Chance program operators have noted that many young women did not pass the GED test the first time they took it; some grew discouraged, dropped out of New Chance, and reenrolled several months later, finally achieving success.

Although women in the experimental group were significantly more likely than controls to have participated in occupational skills training and did so for longer periods (see Chapter 4), virtually identical percentages of young women in both groups (12.5 percent of experimentals and 12.4 percent of controls) had earned a trade certificate or license by the 18-month interview. Why the experimentals' greater participation did not translate into a larger number of credentials is not clear. It is possible that experimentals embarked on training courses that were longer and more demanding than those selected by their control group counterparts. This explanation seems plausible, since a larger proportion of controls who entered training did not have a GED and may not have been able to enter more rigorous training programs.

New Chance had a significant effect on the proportion of young women who had earned college credits toward an A.A. or B.A. degree: 9.8 percent of experimentals and 7.1 percent of controls reported having earned such credits.[5]

B. Subgroup Impacts on Attainment of a GED or High School Diploma

As Table 5.2 makes clear, the program had a significant and positive effect on attainment of a GED or high school diploma for virtually all subgroups of the research sample. There were only three exceptions to this pattern. Obviously, there was no effect for the small number of young women who entered the program with a high school diploma or GED already in hand. Nor was there a significant effect for young women who were not receiving AFDC at baseline (largely because an unusually high percentage of controls in this small subgroup also earned a GED).

---

[5] Fewer members of both research groups received college credits than attended college, but the disparity was somewhat larger for experimentals, of whom 12.5 percent attended college classes but only 9.8 percent earned credits toward a degree. (In comparison, 7.9 percent of the controls reported having attended college, with 7.1 percent having earned college credits.) Research conducted for the New Chance monograph (Quint and Musick, 1994) suggested that a number of New Chance enrollees dropped out of college, sometimes after one semester or less and without having earned any credits. Young women who dropped out of college did so for reasons that often existed in interaction with one another and that fell into five basic categories: academic problems; feelings of being overwhelmed by the need to balance academic, household, and other responsibilities; the negative social environment of college; difficulties negotiating college procedures and regulations (especially those concerning receipt of financial aid); and pregnancy. At one site, a sixth factor – a change in the policy of the welfare agency, which initially allowed AFDC recipients to enroll in four-year colleges but subsequently permitted them to enroll in two-year programs only – accounted for several instances of dropping out.

As expected, no young women in either group had earned a college degree at the 18-month point, since it usually takes a minimum of two years to earn an A.A. However, program staff at many sites noted that young women's progress toward a college degree was slowed by their need to take remedial courses that did not award credit (or in some cases full credit).

The third subgroup for which there was no impact on GED attainment is of greater interest: It comprises young women who entered the research sample reading below the sixth-grade level. Although the data presented in Chapter 4 indicated that the program impact on attendance of education programs was larger for these young women than for more academically skilled sample members, New Chance was usually unable to help these young women attain the level of proficiency needed to pass the GED test.[6] At the other end of the range, the program had a significant and positive effect on GED attainment among young women with reading scores at the tenth-grade level or above; however, the high proportion of controls in this subgroup who received a GED is particularly notable (47.2 percent of controls versus 63.5 percent of experimentals). It appears that, in relative terms, New Chance was most successful in increasing the GED attainment of young women who were in the middle range in terms of academic skills at baseline – young women reading between the sixth- and ninth-grade levels and whose academic skills (and self-confidence) may have needed bolstering but who were within "striking distance" of passing the test. Thus, the 19.2 percentage point impact on GED or high school diploma attainment registered by experimentals reading at the eighth- or ninth-grade level represented an increase of 66.4 percent over the control group average; for experimentals reading at the sixth- or seventh-grade level, the 11.0 percentage point increase marked an increase of 51.4 percent over the control group average.

C. Site Impacts on Attainment of a GED or High School Diploma

Overall, as seen in Table 5.1, 30.0 percent of the controls had earned a high school diploma or GED by the 18-month point. But, as Table 5.3 shows, this proportion varied widely from site to site: Only 16.3 percent of the controls in the Bronx received one of these credentials compared to 46.0 percent of the controls in Portland.

At eight of the New Chance sites (Denver, Inglewood, Jacksonville, Lexington, Minneapolis, Pittsburgh, Portland, and San Jose), experimentals achieved positive and statistically significant gains in high school diploma or GED attainment compared to the control groups at those sites. At three more sites (the Bronx, Chicago Heights, and Salem), the difference also favored the experimentals and was fairly sizeable (8 percentage points or more), but not large enough to be statistically significant given the small sample size at each site. At the remaining five sites, the difference in attainment between experimentals and controls was negligible.[7]

Educational attainment was such a central aim of New Chance that it is worth exploring site variation in GED receipt further to see whether any specific site characteristics were associated with a higher or lower probability of achieving this goal. First, it is important to examine the possibility

---

[6] Research conducted for the New Chance monograph suggests that some exceptionally low-skilled readers may have had learning disabilities that New Chance staff were not equipped to diagnose or treat.

[7] At one of these fives sites (Allentown), the absence of a program effect appears to reflect the unusually high rate at which controls earned a GED (38.7 percent); the proportion of Allentown experimentals to earn this credential was very close to the experimental average for all sites (42.5 percent for Allentown versus 43.1 percent for the demonstration as a whole). At the other four sites where the impact was virtually nonexistent (Chula Vista, Detroit, Harlem, and Philadelphia), experimentals' rates of GED receipt were considerably lower than the rate for experimentals at all sites, while the controls' rates were similar to those of controls at the other sites.

TABLE 5.2

IMPACTS OF NEW CHANCE ON RECEIPT OF A GED OR HIGH SCHOOL DIPLOMA WITHIN 18 MONTHS AFTER RANDOM ASSIGNMENT, BY SUBGROUP

Characteristic and Subgroup at Random Assignment	Sample Size	Experimentals (%)	Controls (%)	Within-Subgroup Impact	p (a)	Between-Subgroups Impact Difference (b)	p (a)
Age (years)						--	0.139
16-17	408	42.1	31.7	10.4 **	0.020		
18-19	996	42.8	32.5	10.3 ***	0.000		
20-22	682	44.0	25.5	18.6 ***	0.000		
Ethnicity						--	0.894
Black, non-Hispanic	1,093	40.2	27.3	12.9 ***	0.000		
Hispanic	465	45.0	30.5	14.6 ***	0.000		
White or other	527	47.2	35.3	11.9 ***	0.002		
Living arrangement						2.3	0.577
Living with mother	710	43.4	29.0	14.4 ***	0.000		
Not living with mother	1,352	42.7	30.7	12.1 ***	0.000		
Number of children						-1.0	0.805
1	1,356	43.6	30.9	12.8 ***	0.000		
More than 1	732	42.2	28.4	13.8 ***	0.000		
Age at first child's birth (years)						1.6	0.694
13-16	840	44.0	29.9	14.1 ***	0.000		
17-19	1,248	42.5	30.0	12.5 ***	0.000		
Age of youngest child (years)						-1.6	0.684
Less than 1	1,119	42.7	30.4	12.3 ***	0.000		
1 or older	965	43.6	29.7	13.9 ***	0.000		
Educational attainment						12.2	0.135
No high school diploma or GED	1,952	39.8	25.9	13.9 ***	0.000		
Had high school diploma or GED	132	92.6	90.9	1.7	0.829		

(continued)

TABLE 5.2 (continued)

Characteristic and Subgroup at Random Assignment	Sample Size	Experimentals (%)	Controls (%)	Within-Subgroup Impact	p (a)	Between-Subgroups Impact Difference (b)	p (a)
Highest grade completed						-4.6	0.280
10th or below	1,384	38.9	27.3	11.6 ***	0.000		
11th or above	701	51.4	35.2	16.2 ***	0.000		
Interval since last attended regular high school						1.7	0.673
More than 2 years	1,097	41.1	27.1	14.0 ***	0.000		
2 years or less	934	45.8	33.5	12.3 ***	0.000		
TABE reading test score (grade equivalent) (c)						-- ***	0.008
Below 6th grade	431	20.5	19.4	1.1	0.792		
6th or 7th grade	482	32.4	21.4	11.0 ***	0.006		
8th or 9th grade	584	48.1	28.9	19.2 ***	0.000		
10th grade or above	585	63.5	47.2	16.3 ***	0.000		
Ever employed						-5.4	0.248
Yes	1,640	43.7	31.7	12.0 ***	0.000		
No	448	41.1	23.6	17.4 ***	0.000		
Prior-year earnings						2.7	0.586
$0-$500	1,660	42.3	28.7	13.6 ***	0.000		
$501 or more	418	46.3	35.3	10.9 **	0.013		
Any AFDC received in household						6.5	0.463
Yes	1,976	42.6	29.1	13.5 ***	0.000		
No	109	51.4	44.3	7.1	0.410		

(continued)

TABLE 5.2 (continued)

Characteristic and Subgroup at Random Assignment	Sample Size	Experimentals (%)	Controls (%)	Within-Subgroup Impact	p (a)	Between-Subgroups Impact Difference (b)	p (a)
Family received AFDC when sample member was growing up						--	0.114
Always	344	40.8	20.5	20.2 ***	0.000		
Sometimes	977	41.0	31.8	9.2 ***	0.001		
Never	751	46.2	31.4	14.9 ***	0.000		
CES-D (depression) Scale (d)						--	0.783
0-15 (not at risk)	979	44.8	31.7	13.2 ***	0.000		
16-23 (at some risk)	539	42.8	27.9	14.8 ***	0.000		
24-60 (at high risk)	566	40.5	29.4	11.2 ***	0.003		

SOURCES: MDRC calculations from New Chance Enrollment Form and survey data.

NOTES: Calculations for this table used data for all 2,088 sample members for whom there were 18 months of follow-up survey data. The reported sample sizes may fall short of this number because of missing or unusable items from some sample members' questionnaires.

The percentages are adjusted using a two-way analysis of covariance procedure controlling for up to 51 kinds of difference in characteristics, other than the characteristic used to define subgroups, before random assignment. The two categories used as factors were research status (i.e., membership in the experimental or control group) and, one at a time, the baseline characteristics indicated. Rounding may cause slight discrepancies in sums and differences.

(a) A two-tailed t-test was applied to each regression-adjusted within-subgroup impact and also, whenever there were two subgroups, to each difference between subgroup impacts. For each characteristic with more than two subgroups, an F-test was applied to the interaction between that characteristic and experimental or control status. The columns labeled "p" are the statistical significance levels of each within-subgroup impact and each between-subgroups difference in impacts: That is, p is the probability that sample estimates are different from zero or from each other only because of random error. Statistical significance levels are indicated as *** = 1 percent; ** = 5 percent; * = 10 percent.

(b) For each characteristic with only two subgroups, the between-subgroups impact difference is the impact for the first subgroup less the impact for the second subgroup. For characteristics with more than two subgroups, a between-subgroups impact difference cannot be calculated, as indicated by dashes in the table.

(c) The test used to measure reading ability was the reading part of the Tests of Adult Basic Education (TABE). Most sites administered the Survey Form of the test, but some administered the full reading test.

(d) The Center for Epidemiological Studies Depression (CES-D) Scale is a widely used measure of depression; scores can range from zero to 60.

TABLE 5.3

IMPACTS OF NEW CHANCE ON RECEIPT OF A GED OR HIGH SCHOOL DIPLOMA WITHIN 18 MONTHS AFTER RANDOM ASSIGNMENT, BY SITE

Site	Sample Size	Experimentals (%)	Controls (%)	Within-Site Impact	p (a)	Between-Sites Impact Difference p (a)
						** 0.045
Allentown	115	42.5	38.7	3.8	0.639	
Bronx	126	27.1	16.3	10.8	0.178	
Chicago Heights	69	35.0	27.0	8.0	0.455	
Chula Vista	127	32.3	32.0	0.3	0.971	
Denver	110	55.0	37.0	18.0 **	0.031	
Detroit	169	30.4	30.6	-0.1	0.985	
Harlem	124	31.8	27.6	4.2	0.601	
Inglewood	131	51.8	21.3	30.4 *** (b)	0.000	
Jacksonville	144	36.1	17.7	18.5 ** (b)	0.012	
Lexington	135	34.9	21.0	14.0 *	0.073	
Minneapolis	121	66.5	42.0	24.4 *** (b)	0.002	
Philadelphia	135	30.4	28.0	2.4	0.748	
Pittsburgh	171	65.2	40.3	24.9 *** (b)	0.000	
Portland	143	58.7	46.0	12.6 *	0.084	
Salem	134	30.1	21.3	8.8	0.252	
San Jose	134	57.0	31.8	25.1 *** (b)	0.001	
Sample size	2,088					

SOURCES: MDRC calculations from New Chance Enrollment Form and survey data.

NOTES: Calculations for this table used data for all 2,088 sample members for whom there were 18 months of follow-up survey data, including those with values of zero for outcomes and New Chance enrollees (i.e., experimentals) who did not participate in the program.

The percentages are adjusted using a two-way analysis of covariance procedure controlling for up to 36 kinds of difference in characteristics, other than site, before random assignment. The two categories used as factors were research status (i.e., membership in the experimental or control group) and site. Rounding may cause slight discrepancies in sums and differences.

(a) A two-tailed t-test was applied to each regression-adjusted within-site impact. An F-test was applied to the interaction between sites and experimental or control status. The columns showing p-values are the statistical significance levels of each within-site impact and each between-sites difference in impacts: That is, p is the probability that sample estimates are different from zero or from each other only because of random error. Statistical significance levels are indicated as *** = 1 percent; ** = 5 percent; * = 10 percent.

(b) Impacts at these sites remained statistically significant after interactions between research status and the 36 non-site characteristics were added to the procedure.

that apparent site differences were really due to differences in the characteristics of sample members at the sites that made them more or less likely to earn a GED (i.e., that some sites achieved significant impacts merely because they enrolled sample members with a higher probability of earning a GED, or vice versa). This may well have been the case because, in addition to the program eligibility criteria all sites used in enrolling participants, some local programs established criteria of their own, excluding, for example, young women reading below a specified level because the programs felt that they could not serve them effectively.[8]

Sample members' characteristics explain some of the variation, but not all of it. When differences in these characteristics from site to site were adjusted for statistically, the impacts on GED attainment in Denver, Lexington, and Portland, while positive in direction, were no longer statistically significant. But five sites continued to have significant and positive GED impacts: Inglewood, Jacksonville, Minneapolis, Pittsburgh, and San Jose.

It is also possible that, along with enrollees' characteristics, certain aspects of program operations at the 16 sites may have been associated with the presence or absence of significant GED impacts. While three such aspects – amount of participation in basic education and GED classes, availability of on-site child care, and enrollees' perceptions of the quality of the education component (as measured by a question on the 18-month survey asking the young women to rate how much they had learned in the education classes) – were examined, none proved of much explanatory value.[9]

It seems likely that a multiplicity of factors (and perhaps different factors at different sites) shaped the behavior of experimentals and controls at a given site, producing or failing to produce impacts on GED receipt in the process. It is also important to note that some factors governing rates of GED attainment were beyond the scope of the program's influence. While the same GED test is administered nationwide, states differ as to what score must be achieved to pass the test. For example, California, Florida, New York, and Oregon require that test-takers attain a minimum score of 40 on each section of the GED test; the other six states with New Chance sites set a minimum score of 35.

---

[8] Enrollees in Harlem and Minneapolis had to read at the sixth-grade level or above. San Jose imposed a 5.5-grade reading level standard initially but subsequently lowered it to fourth-grade or above. Detroit, after struggling to serve many young women with very poor reading skills, eventually decided to impose a 4.5-grade reading floor.

[9] Experimentals attended basic education and GED classes for a significantly greater number of weeks than did controls at all sites except Minneapolis – which nonetheless registered GED impacts.

One might have expected to find impacts at those sites where the difference between the basic education/GED participation levels of experimentals and controls was particularly large, and to find no impacts where the experimental-control difference was small. However, at only one of the three sites where there was a large experimental-control difference in service receipt (Jacksonville) was there also a significant impact on GED attainment, and a significant impact was also found at one of the four sites where the difference was small and impacts might not have been expected (Minneapolis).

One of the nine sites offering regular on-site child care (Pittsburgh) produced positive and statistically significant GED impacts. Four of the seven sites without regular child care arrangements (Inglewood, Jacksonville, Minneapolis, and San Jose) also had positive and significant impacts. It is hard to know what, if anything, this association means.

One of the three sites that participants rated lowest in terms of how much they learned in the education classes nonetheless produced statistically significant GED impacts. Some especially highly rated sites produced impacts, but others did not.

Thus, if two young women, one from the Allentown site and one from the Bronx, took the test and received identical scores of 37 on all sections, the young woman from Allentown would have received her GED, while her counterpart in the Bronx would not have done so. In Oregon, 16-year-olds are permitted to take the test; in Minnesota and New York, with some exceptions, test-takers must be at least 19 years old. Furthermore, localities follow different policies and practices that may affect the speed with which a GED can be acquired. In New York, e.g., it takes two months to be scheduled for the test. If the individual fails to pass, she must wait two months before taking it again – and then she must retake the entire test, not just the parts she failed. Test administration rules may even differ within the same state.[10]

D. GED Receipt and Program Participation

As noted previously, the preceding analyses of GED impacts for the aggregate sample and for sites and subgroups all measured the impact of the *increment* in basic education/GED classes and other services that New Chance provided over and above what controls received; they did not speak to the value of education services in and of themselves (as would have been the case if controls had not received these services). It was possible to gain some insight on the latter issue (although not to arrive at definitive answers) by ascertaining whether young women who participated more in New Chance in general, and in basic education/GED classes in particular, were also more likely to earn a GED.

This examination was confined to women in the experimental group, who were divided into four groups. One group included those who did not participate in New Chance at all; the other three, of approximately equal size, divided the enrollees by their level of overall program participation. The proportion of women who earned a GED in each of these groups appears below:

	Received a GED (%)
Zero hours in New Chance	18.5
Bottom third (1 to 127 hours)	20.8
Middle third (127 to 378 hours)	49.5
Top third (more than 378 hours)	67.3

This suggests that, in terms of GED receipt, participating in New Chance only a little was not very different from not participating at all. Above that minimal level, however, there was a positive correlation between participation hours and GED attainment: The greater the number of hours, the higher the level of GED attainment.

These descriptive findings, it must be emphasized, are not impacts, and inferences must be drawn with extreme caution. It may be that if young women could have been induced to stay in New Chance longer, the program would have been more effective in helping them attain GEDs. But it is worth recalling that young women who *did* register more participation hours were generally more advantaged at baseline, educationally and otherwise, than their counterparts who were less active in the program (see Chapter 3). Furthermore, the general direction of causality in the relationship is unclear – i.e., women may have earned a GED and subsequently entered a Phase II component, so that their hours of program participation were greater for this reason.

---

[10] Of course, experimentals and controls in a community were subject to the same rules regarding testing conditions and passing scores. But these conditions may help to explain why sites that operated high-quality education components may have been less successful in achieving GED impacts than sites with lower-quality education services.

The pattern of GED receipt in relation to hours of basic education or GED instruction was different:

	Received a GED (%)
Zero hours in basic education/GED instruction	19.6
Bottom third (1 to 47 hours)	35.7
Middle third (48 to 130 hours)	58.0
Top third (more than 130 hours)	47.3

This suggests that the greater the amount of GED instruction received, the better the results – but only to a point, after which the law of diminishing marginal returns came into play. Instruction beyond that point yielded dividends: Young women who received more than 130 hours of classes were ultimately successful in earning a GED. But it took more effort on their part, and in all likelihood on the part of their teachers. It seems likely that those who were in a position to obtain a GED relatively quickly (i.e., who had the requisite cognitive skills and whose other problems were manageable) did so.[11]

III. Impacts on Literacy

To measure academic achievement, experimentals and controls were administered the reading section of the Survey Form of the Tests of Adult Basic Education (TABE) as part of the 18-month follow-up.[12] This 30-item test assesses an individual's vocabulary and her ability to comprehend written materials (e.g., by identifying the main idea of a passage, drawing inferences, etc.).

---

[11] As already noted, these conclusions were based on comparisons of outcome levels by "dose" of New Chance within the experimental group and were not based on comparisons with corresponding control group outcomes. However, the story did not change substantially when a somewhat more complex "instrumental variables" approach was taken to the question of the relationship between amount of instruction and GED attainment.

This approach attempted to remove a probable correlation between amount of instruction and the unexplained part of GED attainment from the regression of GED attainment on amount of instruction. Among sample members with the same baseline characteristics, those who were more likely to get GEDs after receiving a certain amount of instruction were more likely to want that level of instruction, and also more likely to be given that level of instruction by program operators. Because such correlations would have led to "selection bias" in estimates of effects of instruction on GED attainment, the instrumental variables approach used research status (e.g., membership in the experimental group) along with baseline characteristics to predict the amount of instruction. Because of the random assignment design, such predictions were entirely uncorrelated with the unexplained part of GED attainment and could be used instead of actual attainment, eliminating selection bias from the estimates.

Such estimates showed the same pattern reported above: diminishing marginal returns to amount of instruction at the highest level.

[12] Administering the mathematics and language sections of the TABE as well would have given a fuller picture of academic achievement, but time constraints made this infeasible.

Table 5.4 indicates that experimentals and controls had virtually identical TABE reading scores at follow-up: On average, members of both research groups read at the 7.8-grade level. The distribution of reading scores was also similar for both groups.[13] There were no subgroups of the research sample for which the program produced a statistically significant impact on reading scores and no site effects on literacy scores worthy of note.

Program guidelines called for New Chance sites to assist young women with poor reading skills at baseline, and for whom a GED appeared unrealistic, to improve their reading scores by two grade levels. Many young women did improve their reading skills over time: Among experimentals reading below the sixth-grade level at entry into the research sample, 35.9 percent read at the sixth-grade level or higher, and 11.8 percent read at the eighth-grade level or higher, at follow-up. However, there was no statistically significant difference between experimentals and controls in this regard: Both groups made gains of similar magnitude.

Why did New Chance produce impacts on GED receipt but not on TABE scores? Several answers to this question can be offered, although all are speculative. First, however, it should be noted that New Chance is not unique in this regard. Similar results – i.e., impacts on GEDs but not on literacy – were found in a recent study of the implementation and effects of basic education in the California Greater Avenues for Independence (GAIN) Program, a welfare-to-work program for adult AFDC recipients (Martinson and Friedlander, 1994).[14]

One possible explanation for the discrepancy between impacts on GED attainment and those on literacy is that experimentals' reading ability did improve, but that gains had dissipated by the 18-month interview. This could have happened if, once having left New Chance, experimentals no longer read as much as they had while they were in the program. In this regard, it is relevant to note that New Chance seems to have had no impact – at least as measured on the survey – on the young women's literacy-related habits. While parenting instructors at the program sites sometimes

---

[13] The average TABE score at the 18-month follow-up was actually somewhat lower than the score at baseline: the equivalent of a 7.8 versus an 8.1 reading level, respectively. This apparent decline may have been an artifact of testing conditions or of the specific form of the TABE that was administered.

At baseline (just prior to random assignment), sample members took the test in a relatively quiet environment at the New Chance site; at the follow-up, they took the test in their own homes, about midway through the survey, with their children and other distractions present. Motivation may also have been a factor explaining the different scores: At baseline, sample members may have felt impelled to do as well as they could to gain admission to New Chance, whereas at the follow-up, they had no incentive to pay close attention and to answer as many questions as possible correctly.

There are four forms of the TABE: Easy (E), Moderate (M), Difficult (D), and Advanced (A). The form administered to an individual depended on her reading score at baseline and the test form administered at that point. More than half (53.0 percent) of the sample members were given Form A at the 18-month follow-up point; only 2.6 percent were given Form E. The internal consistency reliabilities of the TABE were similar and acceptably high across the four forms, ranging from .84 (for Forms M and D) to .88 (for Form E).

[14] The study of GAIN basic education found that the program was successful in increasing GED receipt for program registrants in all five counties that were in the impact study. In four of these counties, the impacts were statistically significant. However, in only one of these counties did GAIN produce large and statistically significant impacts on literacy, as measured by the Test of Applied Literacy Skills (TALS).

TABLE 5.4

IMPACTS OF NEW CHANCE ON LITERACY AT 18 MONTHS AFTER RANDOM ASSIGNMENT

Outcome	Experimentals	Controls	Difference	p (a)
Average TABE reading score at follow-up	748.7 (b)	748.3 (b)	0.4	0.792
Distribution of reading levels at follow-up (c) (%)				
7th grade or below	41.4	41.7	-0.4	0.843
8th or 9th grade	30.6	28.5	2.1	0.320
10th or 11th grade	9.6	11.9	-2.3	0.110
12th grade or higher	18.4	17.9	0.5	0.751
Sample size	1,374	672		

SOURCES: MDRC calculations from New Chance Enrollment Form and survey data.

NOTES: Calculations for this table used data for all 2,088 sample members for whom there were 18 months of follow-up survey data, including those with values of zero for outcomes and New Chance enrollees (i.e., experimentals) who did not participate in the program. The reported sample sizes may fall short of this number because of missing TABE scores.

The averages or percentages are adjusted using linear analysis of covariance procedures controlling for up to 51 kinds of difference in characteristics before random assignment. Rounding may cause slight discrepancies in sums and differences.

(a) A two-tailed t-test or likelihood-ratio chi-square test was applied to each difference between experimental and control group outcomes. The column labeled "p" is the statistical significance level of the difference between experimental and control group outcomes: That is, p is the probability that average outcomes are different only because of random error. Statistical significance levels are indicated as *** = 1 percent; ** = 5 percent; * = 10 percent.

(b) The test administered was the reading part of the Tests of Adult Basic Education (TABE), Survey Form, a 30-item test of reading vocabulary and reading comprehension. The scores shown are equivalent to a 7.8-grade reading level.

(c) Using a multinomial logit estimator, the distributions for experimentals and controls in these mutually exclusive categories were compared and found not to be statistically significantly different from one another.

encouraged the young mothers to stimulate their children's verbal skills by reading aloud to them, the data indicate that experimentals were about as likely as controls to do this: about 57 percent of experimentals and about 54 percent of controls read to their children at least three times a week (not shown in tables). Children of experimentals had almost the same number of books as children of controls: an average of 21.9 books for children of experimentals and 22.6 for children of controls who were 3 to 6 years old. Members of the two research groups also reported that their households regularly received the same average number of magazines, 1.2. On all three measures, experimental-control differences were not statistically significant.

Another possible answer is that the GED and the TABE measure different things. To earn a GED, an individual must master a specific body of information that is tested on the GED test. The GED "test" is actually a 7 1/2-hour battery of tests that are designed to measure what graduating high school seniors in the United States are expected to know. The five component tests correspond to the general framework of high school curricula: writing skills, social studies, science, interpreting literature and the arts, and mathematics. Although the test developers assert that the ability to read, comprehend, and analyze written material is a skill needed for all five tests, there is no section that taps reading ability per se. The TABE Survey Form, in contrast, is a much narrower measure of reading skills.

A third, and related, hypothesis is that much of the instruction at the New Chance sites was geared toward preparing young women to pass the GED test. Once a young woman could read well enough to understand the questions asked on the test, her time in education classes was spent learning the specific subject matter tested, not in further improving her reading.

A final possible explanation is that New Chance increased GED attainment in large measure by increasing experimentals' opportunities to take the GED test. In other words, experimentals who read well enough to take the test and who had the requisite amount of subject knowledge to pass it were helped and pushed by New Chance staff to take the test, while controls who were equally academically able did not receive comparable assistance or encouragement.

Whatever the explanation – and several may hold true simultaneously, or different explanations may apply to different sites – New Chance had no measured effect on the literacy of program enrollees. This finding is especially disturbing to the extent that employers value not just education credentials but also the underlying skills these credentials are intended to represent.

IV. New Chance and Other Programs Compared

Program impacts on literacy were not measured in LEAP, JOBSTART, and Project Redirection. Data on educational attainment and achievement were not reported for the dropout subgroup in the Teenage Parent Demonstration; for the full sample, the demonstration had no significant impact on GED receipt.[15] In LEAP, the GED attainment data were highly preliminary.

To reiterate the point made in Chapter 4: Even when information is available, it is often difficult to compare the results of these programs with those of New Chance because of differences in the program models, the populations served, the length of follow-up, and the program context. In Project

---

[15] Personal communication from Ellen Kisker, Mathematica Policy Research, March 7, 1994.

Redirection, e.g., 20 percent of the experimental group members who were out of school at baseline had obtained a high school diploma or GED by the 24-month interview, compared to 11 percent of teens in the comparison group (Polit and White, 1988). However, the population served by Project Redirection was two and a half years younger, on average, than that enrolled by New Chance.

JOBSTART, which was directed toward male and female high school dropouts in approximately the New Chance age range, provides the closest comparison to New Chance. In JOBSTART, 35.5 percent of the young mothers in the experimental group and 14.2 percent in the control group were reported to have received a GED or high school diploma within a 24-month follow-up period (Cave and Doolittle, 1991). In New Chance, it will be recalled, the figures were 43.1 percent for experimentals and 30 percent for controls within 18 months. Thus, one might conclude that, while the absolute percentage of experimentals who earned a GED was somewhat greater in New Chance, the *impact* (i.e., experimental-control difference) was larger in JOBSTART (21.3 percentage points versus 13.1 percentage points in New Chance), principally because New Chance controls were so much more likely than JOBSTART controls to have earned this credential during 18 and 24 months of follow-up, respectively.

There were, however, subtle differences in the underlying populations served by the two programs that undermine the comparability of these results. The New Chance sample included a small percentage (6.3 percent) of young women who were high school graduates or GED holders upon entry into the research; they were included in the 43.1 percent statistic. JOBSTART enrolled dropouts exclusively and was targeted toward youth reading below the 8th-grade level, although it is estimated that about 20 percent of program enrollees were admitted under an eligibility "window" that permitted better-skilled readers to enroll. (Baseline reading scores were not measured for all JOBSTART enrollees.)

When adjustments were made to make the samples more comparable, a more complex picture emerged.[16] The following table presents impacts on attainment of a GED or high school diploma in New Chance and for young mothers in JOBSTART, by subgroups defined on the basis of reading ability at random assignment:

	New Chance				JOBSTART			
Baseline Reading Level	Experimentals (%)	Controls (%)	Difference	p	Experimentals (%)	Controls (%)	Difference	p
Below 6th grade	15.2	14.3	1.0	0.835	30.1	23.0	7.1	0.488
6th−7th grade	28.3	16.4	11.8***	0.007	34.5	20.0	14.5*	0.057
8th grade or higher	52.2	33.3	18.9***	0.000	48.4	28.8	19.6	0.110

Neither program was successful in producing statistically significant impacts for the very poorest readers, although the proportion of JOBSTART experimentals who read below the sixth-grade level and earned a GED was higher than the corresponding percentage of New Chance experimentals. For those reading at the sixth- or seventh-grade levels, both programs produced statistically significant impacts, and these were of comparable magnitude (11.8 percentage points in New Chance and 14.5 percentage points in JOBSTART). New Chance registered a statistically significant impact for the most able readers (those with reading scores at entry at the eighth-grade level or higher), while the JOBSTART impact just missed statistical significance, largely because of the very small number of young women in this group.

---

[16] The New Chance sample was adjusted by excluding young women who had a high school diploma or GED at baseline. The JOBSTART sample was adjusted by excluding young women who lacked a baseline reading score.

Even with these refinements, care must be taken in interpreting the data for still another reason: One cannot conclude that the same people who volunteered for one program might have volunteered for the other. New Chance applicants might have been especially attracted to a program geared exclusively toward young mothers. Had a program like JOBSTART been available to them, they might or might not have applied, and might or might not have achieved impacts comparable to those registered in JOBSTART. The rather strong disparity between rates of GED receipt for controls in the two demonstrations who were especially poor readers (14.3 percent in New Chance and 23.0 percent in JOBSTART) suggests that the two groups of young mothers were not, in fact, interchangeable.

Finally, JOBSTART and New Chance were mounted in different communities (for the most part) and at different times. Contextual factors, including those that changed over time, must also be taken into account in trying to compare the effectiveness of different interventions.

CHAPTER 6

IMPACTS ON LIVING ARRANGEMENTS, FERTILITY, AND HEALTH

I. Introduction

The path from welfare receipt to economic self-sufficiency is rarely straight and easy for disadvantaged young women who become mothers as teenagers. Even among those with strong intentions to leave the welfare rolls – and even among those who manage to attain their GED – a variety of factors typically hinder even and steady progress toward their goal.

A. Barriers to Self-Sufficiency

Some of the obstacles reflect characteristics of the young women themselves, such as low reading skills or inadequate family support. Others result from circumstances beyond their control, such as housing problems that disrupt regular attendance in school or training programs. Still others are the consequence of decisions they make along the way, such as whether to have another baby in the relatively near future.

The New Chance program was specifically designed to address and diminish many of these barriers. Several components (e.g., life skills and family planning) and features (e.g., strong case management) were incorporated into the model to strengthen participants' ability to become self-sufficient, i.e., to help them secure needed services (such as housing assistance), make informed decisions (e.g., about contraception), and enhance their personal resources (e.g., their health and self-confidence).

This chapter examines program impacts on several factors that could affect both short-term progress toward – and eventual attainment of – improved economic and personal outcomes. Specifically, it looks at the effect of the New Chance program in four areas: living arrangements, fertility and family planning, health, and psychological well-being.

B. A Preview of the Findings

The results summarized in this chapter suggest that New Chance did *not* have the hypothesized positive effects on most of the factors examined. The majority of women in the research sample became pregnant after baseline (i.e., after random assignment), and more than one-fourth had another baby during the follow-up period. The two research groups had similar rates of post-baseline births, but a higher rate of post-baseline pregnancy was observed among the experimentals (57.0 percent) than among the controls (53.0 percent). The experimentals also had a higher rate of abortions than did controls (14.9 percent versus 11.1 percent, respectively). Moreover, the experimentals continued to be at higher risk of another pregnancy: They were less likely than controls to be contracepting regularly at follow-up. There were some indications that experimentals were somewhat more likely than controls to be deliberately planning closely spaced births. For example, a somewhat higher percentage of experimentals than controls had a *planned* post-baseline pregnancy and significantly more intended to have another child in the next 13 to 48 months.

The findings indicate that a higher percentage of women in the experimental group (22.8 percent) than in the control group (19.6 percent) were living with a husband or partner at follow-up; conversely, the control group (34.8 percent) was more likely than the experimental group (28.2 percent) to be living with a parent or grandparent at follow-up. This impact – which could have implications for the women's employment, fertility, and child care arrangements – possibly reflects program assistance with housing for participants whose living arrangements were in crisis. Interestingly, the program's impact on post-baseline pregnancies occurred only in conjunction with living with a husband or partner at follow-up. It is also worth noting that the program's impact on GED attainment was not restricted to women who avoided a pregnancy: Experimentals were more likely than controls (21.3 percent versus 12.9 percent) to have obtained their GED *and* to have had a subsequent pregnancy.

Based on self-reports, there were no group differences with respect to any measured health outcomes, including the incidence of sexually transmitted diseases (STDs), use of drugs, and use of alcohol. In terms of emotional well-being, there were fairly high reported levels of stress and depression in both groups. While the experimentals and controls had similar scores on a commonly used depression scale at follow-up, and while both groups had lower average depression scores than at baseline, the improvement was somewhat greater among the controls. However, women in the experimental group (5.4 percent) were less likely than those in the control group (8.1 percent) to say that they had no one to turn to for emotional support, and they also reported greater satisfaction with the social support available to them.

On the whole, the findings in this chapter raise concerns about the experimental group's fertility behavior, given their higher rate of subsequent pregnancy and their ongoing failure to protect against another pregnancy. However, post-baseline pregnancies appeared to be occurring within a different context for experimentals – a context shaped in part by their living arrangements and their attainment of a GED certificate. It may therefore be premature to anticipate how the higher rate of pregnancies will affect eventual self-sufficiency.

II. Impacts on Living Arrangements and Marriage

At random assignment, the majority of young mothers were teenagers and were still living with a parent or another adult relative. It might be expected that the living arrangements of many of these young women would have changed since random assignment – not only because most of them were no longer teenagers at the 18-month follow-up point, but also because there is considerable evidence of volatility of living arrangements among young adults (DaVanzo and Goldsheider, 1990; Thornton, Young-DeMarco, and Goldsheider, 1993) and among poor families with children (Hunter and Ensminger, 1992; Long, 1992).

The New Chance programs did not explicitly seek to alter participants' living arrangements. Yet, given the intensive and comprehensive nature of the program model, it seems possible that the program *could* have affected household composition. For example, if program staff assisted them with housing, some participants might have gotten an opportunity to live independently of their parents.[1] Another possibility is that young mothers' housing patterns might have been more likely to change when they attained their GED or high school diploma – e.g., if that credential was perceived as a symbol of their having passed into adulthood. If this occurred, the higher rate of GED attainment among experimentals could have resulted in a lower rate of their living at home with their parents.

The living arrangements of poor young mothers are currently the focus of heated policy debate. Proposals to require teenage mothers on welfare to live with a parent, while primarily intended to deter teenagers from having a child in order to establish their own welfare-supported households, may also reflect the belief that living arrangements influence young mothers' behavior and options in various ways. For example, young women who live with their own mothers or grandmothers often have a convenient and inexpensive child care arrangement available to them, which could affect their ability to pursue self-sufficiency-oriented activities.[2]

Living arrangements involving cohabitation with a male partner are also likely to influence many aspects of young mothers' lives. Women who get married or live with a boyfriend may have (or believe they have) a route to self-sufficiency that does not require them to be employed. Moreover, having a live-in partner could increase the likelihood of another pregnancy – either because of greater opportunity for intercourse or greater desire (or pressure) to have another baby. Thus, in interpreting program impacts – or their absence – in other areas reviewed in this report (e.g., fertility, employment, and welfare receipt), it is important to know whether the experimental and control groups had similar living arrangements. This section examines whether participation in the New Chance program had any effects on the living arrangements, household composition, and marital status of the young mothers 18 months after random assignment.

As shown in Table 6.1, there were significant differences between experimental and control group members with respect to living arrangements at the time of the 18-month follow-up interview. Fewer young mothers in either the experimental or control group were living with parents or grandparents at follow-up than at baseline, when about half (48.5 percent) were living with an adult relative. However, fewer women in the experimental (28.2 percent) than in the control group (34.8 percent) were living with a parent or grandparent at follow-up.[3] In contrast, fewer controls (19.6

---

[1] The programs intervened to help participants with housing problems primarily when staff perceived that the young women were in crisis (e.g., when a parent evicted them or when there was concern about physical or sexual abuse in the household) or, at some sites, when participants lived in neighborhoods where their safety was threatened.

[2] Interestingly, however, some researchers have found that such arrangements do not necessarily facilitate self-sufficiency. For example, it has been found that family-provided child care fails to increase the labor force participation of previously unemployed young mothers (Blau and Robins, 1989; Parish, Hao, and Hogan, 1991).

[3] Subgroup analyses (not shown in tables) revealed that the experimental-control group difference in living arrangements was fairly pervasive across subgroups and sites. Interestingly, 36.9 percent of sample members who were living with a parent or grandparent at follow-up, compared to 10.6 percent of those who were not, said "yes" to the question: "Do you have people living with you – relatives or friends – who you wish weren't there or who you wish you didn't have to live with?"

TABLE 6.1

IMPACTS OF NEW CHANCE ON HOUSEHOLD LIVING ARRANGEMENTS AT 18 MONTHS AFTER RANDOM ASSIGNMENT

Outcome	Experimentals	Controls	Difference	p (a)
At follow-up, living with (b) (%)				
Husband or partner, without parent or grandparent	22.8	19.6	3.2 *	0.085
Parent or grandparent	28.2	34.8	-6.5 ***	0.002
Children only	35.8	33.9	2.0	0.353
Other (c)	13.1	11.8	1.3	0.403
Living without any children (%)	2.4	2.2	0.3	0.707
Average number of household members	4.1	4.2	-0.1 *	0.084
Average number of children in household, including own and stepchildren	1.6	1.6	0.0	0.604
Marital status at follow-up (d) (%)				
Married	8.1	7.9	0.2	0.868
Never married	84.6	83.7	0.9	0.442
Other	7.2	8.3	-1.1	0.242
Sample size	1,408	680		

SOURCES: MDRC calculations from New Chance Enrollment Form and survey data.

NOTES: Calculations for this table used data for all 2,088 sample members for whom there were 18 months of follow-up survey data, including those with values of zero for outcomes and New Chance enrollees (i.e., experimentals) who did not participate in the program.

The averages or percentages are adjusted using linear analysis of covariance procedures controlling for up to 51 kinds of difference in characteristics before random assignment. Rounding may cause slight discrepancies in sums and differences.

Distributions may not total 100.0 percent because of rounding.

(a) A two-tailed t-test was applied to each regression-adjusted difference between average experimental and control group outcomes. The column labeled "p" is the statistical significance level of the difference between experimental and control group outcomes: That is, p is the probability that average outcomes are different only because of random error. Statistical significance levels are indicated as *** = 1 percent; ** = 5 percent; * = 10 percent.

(b) Using a multinomial logit estimator, the distributions for experimentals and controls in these mutually exclusive categories of living arrangements were compared and found to be statistically significantly different from one another.

(c) Includes living with friends, living with relatives other than parents or grandparents, and living alone with no children.

(d) Using a multinomial logit estimator, the distributions for experimentals and controls in these mutually exclusive categories of marital statuses were compared and found not to be statistically significantly different from one another.

percent) than experimentals (22.8 percent) were living with a husband or partner at follow-up.[4] About two-thirds of these women were living with partners rather than husbands. About one-third of the women in each group were living alone with no other adults in the household.

An examination of patterns of moves from baseline to follow-up (not shown in tables) reveals that, among those sample members who lived with their mothers at baseline, experimental group women were somewhat more likely than control group women to have moved *away* in the 18-month period (18.7 percent versus 15.4 percent, respectively). Moreover, among sample members who had been living with a partner at baseline, controls (34.2 percent) were substantially more likely than experimentals (18.5 percent) to have moved back *in* with their mothers during the follow-up period. Experimental group women who had stopped living with a partner since baseline were, in contrast, more likely subsequently to live on their own. These patterns are consistent with the possibility that some young women established their own residence as a result of housing assistance from program staff.

Given the current debate about requiring young mothers to remain in their parents' home as a condition for welfare receipt, the program impact on living arrangements is of particular interest – especially in light of some evidence suggesting that adolescent mothers who remain with their parents are at less of a disadvantage educationally than those who move away from home (e.g., Furstenberg, Brooks-Gunn, and Morgan, 1987; Scheirer, 1983; Kellam, Ensminger, and Turner, 1982). However, the results of the present study are not consistent with these other findings, perhaps because most of the young mothers in the New Chance sample were no longer teenagers. For example, across both research groups, 41.5 percent of the women living with a parent or grandparent at follow-up had a GED or high school diploma, compared to 44.4 percent of the women who were living with a partner or husband and 45.6 percent of those who were living alone with their children.[5] (Moreover, as was shown in Table 5.2, the percentages of women with a GED or high school diploma at follow-up were nearly identical for women who had and had not been living with their mothers at baseline.)

Returning to Table 6.1, only a small minority of women in each group (about 2 percent) were living without *any* of their own children at follow-up, although about 8 percent of each group had given birth to at least one child who did not live in the same household with them. The young mothers were living with 1.6 children, on average, at the time of the 18-month interview. The two groups did not differ significantly on any of the measures relating to the living arrangements of their children. However, because of the group differences in the mothers' living arrangements, the household size of controls was slightly – but significantly – larger than that for experimentals (4.2 versus 4.1 people, respectively).

Table 6.1 also shows that the marital status of the two groups at follow-up was comparable. The vast majority of women in both groups (about 84 percent) had never been married, reflecting a decrease from the 90.1 percent who were never married at baseline. Among the women who had ever been married, about half (8.0 percent of the sample) were still married and living with a spouse at follow-up, while the remaining half were either separated, divorced, or widowed.

---

[4] About 2.3 percent of the sample were living with both a husband/partner *and* a parent or grandparent. These cases were classified as living with a parent or grandparent.

[5] Chapter 9 examines a wide range of outcomes for women in different living arrangements at follow-up.

III. Impacts on Fertility and Contraception

When they applied to the New Chance program, the majority of young mothers had only one child, said they expected to have no more children, and reported that they were using birth control (most often, oral contraceptives) to postpone or prevent further childbearing. Yet many studies have found that a high percentage of teenage mothers have an early repeat pregnancy. Reports from large-scale surveys have generally found that about 40 percent of teenage mothers become pregnant again within 24 months of delivering their first child (Koenig and Zelnik, 1982; Mott, 1986). In a study of teenage mothers on welfare in the Chicago area, Mosena (1986) found that, among school dropouts, nearly 40 percent of the young women had a second *birth* within 24 months of their first birth. High rates of subsequent pregnancy and birth are of considerable concern, given the evidence that having a second child further reduces teenage mothers' participation in school, training, and employment (see, e.g., Furstenberg, Brooks-Gunn, and Morgan, 1987; Polit, Quint, and Riccio, 1988; Horwitz et al., 1991).

Most programs for teenage mothers offer family planning services, and most have the postponement of subsequent pregnancies as an explicit goal. Yet there is no evidence that any major teen parent program has been successful in achieving this goal. For example, among school dropouts in Project Redirection (a demonstration that was described briefly in Chapter 1), more than half of both the experimental and comparison groups had a subsequent pregnancy within 24 months after baseline; and at the five-year follow-up of the Project Redirection sample, women in the experimental group had actually given birth to a significantly higher average number of children (Polit, Quint, and Riccio, 1988). In the Teenage Parent Demonstration, about 68 percent of the sample of young welfare mothers had a subsequent pregnancy by 28 months after baseline, with similar rates in the experimental and control groups (Maynard, Nicholson, and Rangarajan, 1993).

According to the New Chance model, program staff were explicitly expected to discourage early subsequent pregnancies among program participants, partly because it was believed that an early pregnancy likely would disrupt full and continuous program participation – and likely would also interfere with the pursuit of other longer-term goals. Several components of the New Chance model were designed to help young mothers make sound decisions regarding contraception and childbearing. Family planning classes were offered at all sites, and some sites offered contraception directly.[6] Life skills classes emphasized the importance of decision-making skills and also sought to empower young women in various ways, including enhancing their control over reproductive events. Because repeat pregnancy had been a concern in the Project Redirection demonstration, family planning issues were addressed in technical assistance conferences with New Chance staff prior to implementation of the program. While programs did not discourage further childbearing per se – nor did they directly encourage abortion as a means of postponing childbearing – program staff (at least in theory) endorsed postponement of further pregnancies until the young mothers had made some progress toward self-sufficiency.[7]

---

[6] It should be noted that the period of time under scrutiny – follow-up interviews were conducted between 1990 and 1992 – was a period when there were many changes nationally in contraceptive practices. For example, shortly after the New Chance program got under way, Norplant became available as a contraceptive option to women in the United States, followed by the availability of Depo Provera. (Both of these are highly effective, long-acting hormonal methods that do not require ongoing user attention. Norplant is an implant inserted in the upper arm, providing up to five years of contraceptive protection; Depo-Provera, which involves an injection, provides up to three months of protection.) Moreover, increasing awareness of acquired immunodeficiency syndrome (AIDS) and other sexually transmitted diseases has led to increased use of condoms and a corresponding decline in use of the pill in the past decade (Catania, 1993; Pleck, Sonenstein, and Ku, 1993; Mosher, 1990).

This section examines program impacts on post-baseline fertility and contraception. Given the program model, it was hoped that young women in the program group would be more likely than controls to use effective contraception, and would be less likely to have experienced a post-baseline pregnancy. Program impacts on fertility and contraceptive behavior are first examined for the aggregate sample, and then for subgroups of sample members and for sites. Program effects on subsequent pregnancy and GED attainment considered simultaneously are also examined.

A. Aggregate Impacts on Fertility and Contraception

None of the women in the sample reported that they were pregnant at baseline – indeed, this was a criterion for eligibility for the program. Yet, as shown in Table 6.2, more than half the young women in each group had a pregnancy that began between baseline[8] and the 18-month interview; more than 10 percent of the sample had two or more pregnancies; and more than 25 percent gave birth during the follow-up period. On average, about 16 months elapsed between the date of random assignment and the onset of the first post-baseline pregnancy.

A comparable percentage of women in the experimental and control groups had another baby during the 18-month follow-up period.[9] However, a significantly higher percentage of women in the experimental group (57.0 percent) than in the control group (53.0 percent) had a post-baseline pregnancy. The program impact on the incidence of abortion was also significant: 14.9 percent of the

---

[7] A survey question explicitly asked respondents to rate the extent to which the program emphasized postponing another pregnancy. On a scale of 0 ("not at all") to 10 ("the most possible"), the mean rating was 7.2. This average suggests that many young mothers were aware of a pregnancy-postponement message, but that the message was not perceived to be as powerful as it might have been.

[8] In some cases, pregnancy did occur shortly before baseline. This might have occurred if an applicant did not realize she was pregnant when she applied, or if she failed to reveal the pregnancy so that she could enroll in the program.

[9] Receipt of prenatal care for post-baseline pregnancies was similar in both groups, with most young mothers whose pregnancies resulted in a live birth reporting that they received care in either the first (67.8 percent) or second (24.5 percent) trimester; 5.0 percent failed to receive any prenatal care. The experimental and control groups were also similar with respect to the infants' birthweights: In each group, the average birthweight was 7.1 pounds. Of mothers with a post-baseline birth, 8.8 percent delivered a low-birthweight infant (under 5.5 pounds); in comparison, 9.6 percent of mothers under age 20 in the National Longitudinal Survey of Youth sample had a low-birthweight infant (Mott and Quinlan, 1991).

TABLE 6.2

NET IMPACTS OF NEW CHANCE ON PREGNANCY AND CHILDBEARING AT OR WITHIN 18 MONTHS AFTER RANDOM ASSIGNMENT

Outcome	Experimentals	Controls	Difference	p (a)
During follow-up, had 1 or more (%)				
Pregnancy	57.0	53.0	4.0 *	0.083
Birth	28.4	26.2	2.2	0.297
Abortion	14.9	11.1	3.8 **	0.017
Miscarriage	8.4	9.5	-1.1	0.407
Number of pregnancies during follow-up (b) (%)				
0	43.0	47.0	-4.0 *	0.083
1	45.3	43.2	2.1	0.369
2 or more	11.7	9.8	1.9	0.194
During follow-up, had 1 or more planned pregnancies (%)	8.0	6.1	1.9	0.126
During follow-up, had 1 or more unplanned pregnancies (%)	49.5	47.8	1.7	0.471
Average number of months from random assignment date to onset of first pregnancy during follow-up (c)	15.8	16.6	-0.8	0.139
Average total number of live births during follow-up	1.8	1.7	0.0	0.197
Average age of youngest child at follow-up (years)	1.6	1.7	-0.1	0.111
Sample size	1,366	658		

SOURCES: MDRC calculations from New Chance Enrollment Form and survey data.

NOTES: Calculations for this table used data for all 2,088 sample members for whom there were 18 months of follow-up survey data, including those with values of zero for outcomes and New Chance enrollees (i.e., experimentals) who did not participate in the program. The reported sample sizes may fall short of this number because of missing or unusable items from some sample members' questionnaires.

The averages or percentages are adjusted using linear analysis of covariance procedures controlling for up to 51 kinds of difference in characteristics before random assignment. Rounding may cause slight discrepancies in sums and differences.

Distributions may not total 100.0 percent because of rounding.

(a) A two-tailed t-test was applied to each regression-adjusted difference between average experimental and control group outcomes. The column labeled "p" is the statistical significance level of the difference between experimental and control group outcomes: That is, p is the probability that average outcomes are different only because of random error. Statistical significance levels are indicated as *** = 1 percent; ** = 5 percent; * = 10 percent.

(b) Using a multinomial logit estimator, the distributions for experimentals and controls in these mutually exclusive categories of number of pregnancies were compared and found to be statistically significantly different from one another.

(c) These estimates were generated using a Tobit estimator to correct for truncation at the end of the follow-up period.

experimental group, compared to 11.1 percent of the control group, reported a post-baseline abortion.[10]

According to the mothers' reports, the vast majority (about 88 percent) of the post-baseline pregnancies were not planned (although 57.6 percent of the women with unplanned pregnancies reported that they were *not* upset when they discovered that they were pregnant).[11] Interestingly, a somewhat higher percentage of women in the experimental group (8.0 percent) than in the control group (6.1 percent) said that they had *planned* a post-baseline pregnancy, a difference that narrowly missed statistical significance (i.e., as shown in the last column of Table 6.2, there was only a 12.6 percent probability – a p-value of .126 – that this finding was the result of chance, close to the 10 percent probability that signifies statistical significance). The pattern of results suggests that the program may have had some modest effects on the women's sense of control over their reproductive lives: More of the experimental group's pregnancies were planned, and unplanned pregnancies were more likely to have been terminated by abortion. Nevertheless, it is not clear why somewhat more experimentals than controls would have planned a pregnancy. One possibility is that some of the women in the experimental group used pregnancy to escape from failure or disappointment if they were not progressing as had been hoped – or to escape from moving on to training or employment after obtaining their GEDs, as suggested in the in-depth study of 50 participants (Quint and Musick, 1994). Some further possibilities are discussed later in this chapter.

Table 6.2 shows that, overall, women in the two groups had similar childbearing histories: They had given birth to an average of about 1.7 children by the time of the 18-month interview. The average age of the youngest child was somewhat lower in the experimental group (1.6 years) than in the control group (1.7 years); this difference was not statistically significant, but narrowly missed conventional levels of statistical significance.

The 18-month interview also included questions about the young mothers' use of contraception in the two months prior to the interview. Table 6.3 shows that a comparable percentage of women in the two groups – just under 20 percent – were sexually abstinent and not pregnant at the time of the

---

[10] It has been well documented that abortion is underreported in self-report surveys (e.g., Jones and Forrest, 1992). It is possible that experimentals and controls were differentially likely to have "forgotten" a pregnancy that ended in an abortion, although it is not clear what direction such a reporting bias might have taken. If experimentals felt greater pressure to avoid a pregnancy because of program expectations, their pregnancy and abortion rates might have been underreported to a greater degree than those of controls. On the other hand, if experimentals had a greater tolerance for abortion as a means of managing reproductive events (or a greater willingness to discuss their sexuality in general as a result of family planning classes), they might have been more open about reporting abortions even if the rates in the two groups were comparable.

[11] The question regarding pregnancy planning was asked as follows: "Looking back on that pregnancy, would you say that you had planned to get pregnant and were happy; had not planned to get pregnant but weren't upset about it; or had not planned to get pregnant and *were* upset when you found out?" It should be noted that retrospective accounts of the extent to which a pregnancy was planned are likely to be subject to recall biases. Moreover, among young mothers, it is possible that "planning" of a pregnancy is not always done on a conscious level.

TABLE 6.3

IMPACTS OF NEW CHANCE ON BIRTH CONTROL AND FERTILITY EXPECTATIONS AT 18 MONTHS AFTER RANDOM ASSIGNMENT

Outcome	Experimentals (%)	Controls (%)	Difference	p (a)
Birth control status at follow-up (b)				
Sexually abstinent for at least the prior two months, not pregnant	17.8	19.5	-1.7	0.353
Sexually active, contracepting regularly (c)	37.0	41.0	-4.0 *	0.083
Sexually active, not contracepting regularly (c)	30.2	25.2	4.9 **	0.022
Pregnant	15.0	14.3	0.8	0.657
Used a prescription/surgical method of contraception during prior two months (d)	33.5	32.8	0.8	0.734
Had a partner who used condoms during prior two months	37.4	38.9	-1.5	0.500
Future childbearing expectations (b, e)				
Expects to have no more children	53.5	56.1	-2.6	0.432
Expects to have another child within next 12 months	7.2	9.1	-1.9	0.304
Expects to have another child in 13-48 months	21.5	15.7	5.8 **	0.031
Expects to have another child in 49 months or more	17.8	19.1	-1.3	0.613
Sample size	1,366	658		

SOURCES: MDRC calculations from New Chance Enrollment Form and survey data.

NOTES: Calculations for this table used data for all 2,088 sample members for whom there were 18 months of follow-up survey data, including those with values of zero for outcomes and New Chance enrollees (i.e., experimentals) who did not participate in the program. The reported sample sizes may fall short of this number because of missing or unusable items from some sample members' questionnaires.

The percentages are adjusted using linear analysis of covariance procedures controlling for up to 51 kinds of difference in characteristics before random assignment. Rounding may cause slight discrepancies in sums and differences.

(a) A two-tailed t-test was applied to each regression-adjusted difference between average experimental and control group outcomes. The column labeled "p" is the statistical significance level of the difference between experimental and control group outcomes: That is, p is the probability that average outcomes are different only because of random error. Statistical significance levels are indicated as *** = 1 percent; ** = 5 percent; * = 10 percent.

(b) Using a multinomial logit estimator, the distributions for experimentals and controls in these mutually exclusive categories were compared and found to be statistically significantly different from one another.

(c) A respondent who reported using contraception at each intercourse and/or said that she always took a birth control pill when she was supposed to was considered to be contracepting regularly.

(d) Includes tubal ligation, birth control pills, Norplant, Depo Provera, and the IUD.

(e) The question on future childbearing expectations was asked of half the research sample, selected at random.

follow-up interview.[12] A similar percentage of each group (about 15 percent) were pregnant when they were interviewed. However, among the sexually active non-pregnant young women, the groups differed with respect to regularity of contraceptive use. One-fourth of all controls (which represents 38.0 percent of those who were sexually active and not already pregnant) said that they failed to use contraception all the time. Significantly more experimentals (30.2 percent of all experimentals, and 44.9 percent of those who were sexually active and not pregnant) were using contraception irregularly and were therefore at risk of another pregnancy.

Unfortunately, the interview did not ask the women whether they were trying to get pregnant. However, a survey question did ask respondents to rate how upset they would be if they became pregnant in the next month, on a scale of 0 ("not at all upset") to 10 ("the most possible"). Women who were sexually abstinent had average ratings similar to those of women who used birth control regularly (means of 8.2 and 7.9, respectively). In contrast, women who were irregular users of birth control had a mean "upset" rating of 6.6, indicating that they would have been substantially less upset with a pregnancy than other women. Moreover, women who used contraception irregularly were more likely than other women (14.9 percent versus 5.3 percent, respectively) to say that they expected to have another child within the next 12 months, strongly suggesting that at least some of the women who were not regular users of contraception were planning another pregnancy. If this is so, the group differences with respect to contraceptive practices may have been indicative of plans for accelerated childbearing on the part of the experimentals.

Table 6.3 shows that the women's choice of contraceptive methods was similar in the two research groups. About one-third of the women in each group (50 percent of the sexually active, non-pregnant women) had used a medical method of birth control in the previous two months – i.e., a method such as oral contraceptives, Depo Provera, Norplant, an intrauterine device (IUD), or tubal ligation. In both groups, women were somewhat more likely to rely on a partner's using a condom than on their own use of a medically prescribed method of contraception.[13]

When asked about their future childbearing expectations, more than half the women in each group said that they expected to have no more children, as Table 6.3 indicates. However, 7.2 percent of the experimentals and 9.1 percent of the controls said that they expected to have another child within the next year – and this did not include any pregnancies then in progress. A significantly

---

[12] Sexual abstinence was defined as not having had sexual intercourse in the two months prior to the interview.

[13] Among the women who were sexually active and not pregnant at follow-up, the most commonly used methods of contraception were as follows (the percentages total more than 100 percent because many young women used multiple methods): condoms (70.0 percent); birth control pills (44.0 percent); withdrawal (21.0 percent); foam, jelly, or suppositories (11.4 percent); Norplant (7.0 percent); rhythm (7.1 percent); tubal ligations (4.5 percent); and the Depo Provera shot (4.3 percent). Consistent with national trends, this distribution suggests a shift over the 18-month period away from highly effective, female-controlled methods (such as the pill) to a less effective, male-controlled method (condoms) that does, however, also protect against sexually transmitted diseases.

Why One New Chance Participant Did Not Use Birth Control

The following profile of "Virginia," taken from the New Chance monograph (Quint and Musick, 1994), suggests a number of reasons why a young woman might not use contraception, thereby placing herself at risk for another pregnancy.

In the two and a half years following her enrollment in New Chance, **Virginia** had one pregnancy, which resulted in a therapeutic abortion, because she "can't take the pill" and her partner at that time, the father of her children, did not want to use condoms. That man is now in prison; her new boyfriend and she also do not use contraception. When asked what would happen now if she were to get pregnant, she laughed:

> I don't know. I mean, he wants a baby bad, you know, 'cause, like, he always say all his friends got kids and he don't. I told him, I said, "I got two, so you got two. I mean, you want to help take care of mine, you could help." But he want more, but I ain't really ready for it. I want another one when my kids turn five.

That date is two years away, and she explained why she is not using some kind of protection now:

> I mean, I can't take the pill, and he don't like using protection, so what am I going to . . . ? Then, that Norplant that they give you – that don't work. 'Cause I have a friend. When the Norplants first came out, she got one. She's pregnant, she's due in April, and she still got a Norplant in her arm. So I say, I ain't going to waste my time getting one if they don't work.

This statement as well as other details in the interview point to several factors underlying Virginia's failure to use birth control. First is her contraceptive ignorance: She is convinced that Norplant does not work, and her query – "so what am I going to . . . ?" – suggests that she knows little about such over-the-counter methods as foam or the contraceptive sponge. Second is the fact that her boyfriend is pressuring her to have a child by him. Her own motivation to resist this pressure is also weak. She is unemployed and has no clear career goals. Finally, she does not believe that a new baby would disrupt her life plans. She is sure that her friends or her nieces would be able and willing to babysit for her: "So I don't think one more child would have interfered."

higher percentage of those in the experimental group than in the control group said that they expected to have another child within 13 to 48 months.[14] This pattern may lend further support to the notion that experimentals were exhibiting somewhat more planfulness with regard to fertility than controls. Teenage mothers often say that they are planning to have another child "in five years or so" (see, e.g., Polit, 1992b). Five years appears to represent an abstract point in a relatively distant future, a point symbolizing their expectations of when their lives will have improved and stabilized. Thus, the higher percentage of experimentals saying that they expected to have a child in two to four years may indicate a greater degree of specific planning regarding the spacing of children.

---

[14] Responses to the question about when the young women expected to have another child ranged from less than a year from that time to 20 years from then. Among the women who expected to have at least one more child, the average expected time until the birth of the next child was 4.1 years. The women were most likely to say that they expected to have only one more child (70.3 percent); 21.3 percent said that they expected to have two more children, and 8.3 percent said that they expected to have at least three more children.

In summary, although the post-baseline birthrates for the women in the experimental and controls groups were comparable at the 18-month point, the findings indicate that the two groups were behaving in a significantly different fashion with regard to contraception and fertility. The women in the experimental group were more likely than those in the control group to have gotten pregnant after random assignment (and somewhat more likely to have planned their pregnancy), and they were at higher risk of further pregnancies because they did not use contraception regularly. They were also more likely to say that they expected to have another child in two to four years, suggesting differences in intended child-spacing that were consistent with their actual contraceptive behavior.

B. Subgroup Impacts on Post-Baseline Pregnancies

Contrary to the program objectives, New Chance appears to have had a modest (but significant) effect on *increasing* the rate of pregnancies in the post-baseline period for the aggregate sample. This section examines the extent to which the impact prevailed across subgroups.

Table 6.4 shows that the significant program impacts on post-baseline pregnancies were not universal across subgroups – but neither were they confined to one or two subgroups (indeed, there were significant program impacts for 10 of the 40 subgroups examined). Moreover, even though subgroup impacts were not always significant, there were higher pregnancy rates among experimentals than controls in all but two subgroups. It also appears that participation in New Chance may have led to greater homogeneity across subgroups: Within subgroup clusters, the percentage with a post-baseline pregnancy was substantially more similar among experimentals than among controls. For example, among controls, 36.9 percent of those who had a high school diploma or GED at baseline had a post-baseline pregnancy compared to 54.0 percent of those who had not completed school – a 17.1 percentage point difference. Among experimentals, baseline school completion was more modestly related to the post-baseline pregnancy rate (57.4 percent among non-completers compared to 50.5 percent among completers). Among experimentals, only one subgroup (women who were not on welfare at baseline), compared to 10 control subgroups, had a post-baseline pregnancy rate under 50 percent.

Thus, the program's effect on post-baseline pregnancies, while modest, was fairly pervasive.[15] In only one set of subgroups (highest grade completed at baseline) were the *differences*

---

[15] Program impacts on the rate of post-baseline abortions were also pervasive across subgroups, with higher rates among experimentals than controls in all but one subgroup (those reading at the tenth-grade level or above at baseline); the differences were significant in 22 out of 40 subgroups. The significant impacts generally ranged from 4 to 7 percentage points. However, the impact was especially large among those who (continued...)

TABLE 6.4

IMPACTS OF NEW CHANCE ON PREGNANCY WITHIN 18 MONTHS AFTER RANDOM ASSIGNMENT, BY SUBGROUP

Characteristic and Subgroup at Random Assignment	Sample Size	Experimentals (%)	Controls (%)	Within-Subgroup Impact	p (a)	Between-Subgroups Impact Difference (b)	p (a)
Age (years)						--	0.972
16-17	393	65.2	62.3	2.9	0.591		
18-19	967	57.6	53.2	4.4	0.185		
20-22	662	51.2	47.3	3.9	0.334		
Ethnicity						--	0.383
Black, non-Hispanic	1,055	59.0	53.9	5.1	0.112		
Hispanic	455	53.1	54.7	-1.6	0.745		
White or other	511	56.7	49.3	7.4	0.112		
Living arrangement						-0.3	0.950
Living with mother	691	53.2	48.8	4.4	0.275		
Not living with mother	1,307	59.5	54.8	4.7	0.104		
Number of children						-1.7	0.730
1	1,317	53.5	50.0	3.4	0.235		
More than 1	707	63.5	58.4	5.1	0.190		
Age at first child's birth (years)						-4.7	0.326
13-16	813	55.3	54.1	1.2	0.744		
17-19	1,211	58.2	52.3	5.9 **	0.049		
Fertility expectations						3.0	0.534
Expects no more children	1,277	58.0	53.0	4.9 *	0.092		
Expects more children	726	55.6	53.7	1.9	0.625		
Abortion history						-6.5	0.250
Never had an abortion	1,565	54.2	51.5	2.7	0.309		
Had an abortion	457	66.7	57.5	9.2 *	0.064		
Age of youngest child (years)						5.1	0.274
Less than 1	1,084	57.1	50.7	6.4 **	0.044		
1 or older	936	56.9	55.6	1.3	0.711		

(continued)

TABLE 6.4 (continued)

Characteristic and Subgroup at Random Assignment	Sample Size	Experimentals (%)	Controls (%)	Within-Subgroup Impact	p (a)	Between-Subgroups Impact Difference (b)	p (a)
Educational attainment						−10.2	0.293
No high school diploma or GED	1,892	57.4	54.0	3.4	0.158		
Had high school diploma or GED	128	50.5	36.9	13.6	0.147		
Highest grade completed						−11.7 **	0.018
10th or below	1,341	57.5	57.3	0.2	0.932		
11th or above	680	56.1	44.2	11.9 ***	0.003		
Interval since last attended regular high school						4.5	0.341
More than 2 years	1,063	55.8	49.7	6.1 *	0.058		
2 years or less	905	57.8	56.2	1.5	0.661		
TABE reading test score (grade equivalent) (c)						--	0.540
Below 6th grade	417	56.7	51.5	5.2	0.322		
6th or 7th grade	468	55.9	56.1	−0.2	0.971		
8th or 9th grade	560	61.3	52.5	8.8 **	0.046		
10th grade or above	573	54.1	52.1	2.0	0.645		
Ever employed						−4.8	0.403
Yes	1,593	56.2	53.3	2.9	0.263		
No	431	59.8	52.1	7.7	0.129		
Prior-year earnings						−0.4	0.941
$0−$500	1,609	57.7	53.8	4.0	0.129		
$501 or more	405	54.2	49.8	4.4	0.402		
Any AFDC received in household						2.0	0.850
Yes	1,912	57.6	53.7	4.0 *	0.098		
No	109	45.3	43.3	2.0	0.844		

(continued)

TABLE 6.4 (continued)

Characteristic and Subgroup at Random Assignment	Sample Size	Experimentals (%)	Controls (%)	Within-Subgroup Impact	p (a)	Between-Subgroups Impact Difference (b)	p (a)
Family received AFDC when sample member was growing up						--	0.316
Always	327	59.9	51.4	8.5	0.158		
Sometimes	951	58.1	57.6	0.5	0.887		
Never	730	54.3	47.2	7.2 *	0.068		
CES-D (depression) Scale (d)						--	0.709
0-15 (not at risk)	947	55.4	49.1	6.3 *	0.068		
16-23 (at some risk)	526	57.2	54.4	2.8	0.541		
24-60 (at high risk)	547	60.0	57.9	2.1	0.639		
Sample size	2,024						

SOURCES: MDRC calculations from New Chance Enrollment Form and survey data.

NOTES: Calculations for this table used data for all 2,088 sample members for whom there were 18 months of follow-up survey data, including those with values of zero for outcomes and New Chance enrollees (i.e., experimentals) who did not participate in the program. The reported sample sizes may fall short of this number because of missing or unusable items from some sample members' questionnaires.

The percentages are adjusted using a two-way analysis of covariance procedure controlling for up to 51 kinds of difference in characteristics, other than the characteristic used to define subgroups, before random assignment. The two categories used as factors were research status (i.e., membership in the experimental or control group) and, one at a time, the baseline characteristics indicated. Rounding may cause slight discrepancies in sums and differences.

(a) A two-tailed t-test was applied to each regression-adjusted within-subgroup impact and also, whenever there were two subgroups, to each difference between subgroup impacts. For each characteristic with more than two subgroups, an F-test was applied to the interaction between that characteristic and experimental or control status. The columns labeled "p" are the statistical significance levels of each within-subgroup impact and each between-subgroups difference in impacts: That is, p is the probability that sample estimates are different from zero or from each other only because of random error. Statistical significance levels are indicated as *** = 1 percent; ** = 5 percent; * = 10 percent.

(b) For each characteristic with only two subgroups, the between-subgroups impact difference is the impact for the first subgroup less the impact for the second subgroup. For characteristics with more than two subgroups, a between-subgroups impact difference cannot be calculated, as indicated by dashes in the table.

(c) The test used to measure reading ability was the reading part of the Tests of Adult Basic Education (TABE). Most sites administered the Survey Form of the test, but some administered the full reading test.

(d) The Center for Epidemiological Studies Depression (CES-D) Scale is a widely used measure of depression; scores can range from zero to 60.

between subgroup impacts significant. There does not appear to be a clear-cut pattern of significant subgroup impacts on subsequent pregnancies. The pattern of program effects does not suggest a greater program impact on increasing the rate of post-baseline pregnancy in relation to degree of initial disadvantage: In some cases, the impacts were greater among the *less* disadvantaged subgroups (e.g., those who were not depressed and those who had completed more schooling at baseline), but sometimes *more* disadvantaged subgroups (e.g., those who had never been employed before baseline and those living in an AFDC household) had larger impacts.

C. Site Impacts on Post-Baseline Pregnancies

Table 6.5 examines program impacts on post-baseline pregnancies at individual sites. At two sites (Detroit and Portland), the higher rate of pregnancies among experimentals was statistically significant. Although the difference in impacts among sites was not significant, it is worth noting that, at two sites (Denver and San Jose), the program led to a rather sizeable reduction in post-baseline pregnancies, and in the case of San Jose this difference fell just short of achieving statistical significance. When site-level impacts on *un*planned pregnancies were considered, the favorable impact in San Jose was larger (17.6 percentage points), resulting in a difference that was statistically significant (not shown in tables).[16]

Further exploratory analyses were undertaken in an effort to shed light on factors that might help to account for site variation with respect to fertility outcomes – variation such as the significant favorable impact on unplanned pregnancy in San Jose and the significant unfavorable impact in Detroit. If the New Chance programs themselves (rather than characteristics of the participants) affected the fertility behavior of the young mothers, certain dimensions of the program might be especially important influences. Four specific service-related factors were compared among the 16 sites: the amount of family planning services New Chance enrollees received; the amount of life skills classes enrollees received; the degree of emphasis the programs gave to pregnancy postponement, as perceived by enrollees; and the degree to which the family planning component adhered to program guidelines, as noted by MDRC field staff.

These analyses revealed a few interesting patterns. For example, experimentals in both Denver and San Jose (where fertility impacts looked most promising) were especially likely to say that pregnancy postponement was emphasized by New Chance staff, and experimentals at both sites reported higher than average rates of service receipt for both family planning and life skills.[17]

---

[15](...continued)
had had an abortion prior to applying to the program: 28.6 percent of the experimentals and 15.9 percent of the controls in this subgroup had another abortion during the follow-up period (not shown in tables).

[16] When site differences in the characteristics of the young mothers were adjusted statistically (i.e., when adjustments were made so that the applicant profiles at random assignment were comparable across sites), the difference favoring experimentals in San Jose became even larger. This means that the experimental-control group difference in San Jose cannot be explained by the fact that the applicants at that site were a relatively advantaged group.

[17] The MIS data and survey data are inconsistent with regard to receipt of family planning services among the experimentals in San Jose. According to the MIS, experimentals in this site had *lower* than average participation in family planning sessions.

TABLE 6.5

IMPACTS OF NEW CHANCE ON PREGNANCY WITHIN 18 MONTHS AFTER RANDOM ASSIGNMENT, BY SITE

Site	Sample Size	Experimentals (%)	Controls (%)	Within-Site Impact	p (a)	Between-Sites Impact Difference p (a)
Allentown	110	53.8	47.6	6.3	0.530	0.472
Bronx	119	59.5	46.3	13.1	0.178	
Chicago Heights	66	63.9	51.8	12.2	0.350	
Chula Vista	122	42.5	41.5	0.9	0.920	
Denver	108	45.6	54.6	-9.0	0.367	
Detroit	158	63.2	44.1	19.1 **	0.022	
Harlem	122	58.0	53.9	4.1	0.674	
Inglewood	128	58.2	46.6	11.6	0.204	
Jacksonville	142	52.5	52.5	0.0	0.999	
Lexington	131	61.1	60.6	0.5	0.957	
Minneapolis	122	58.7	47.7	11.0	0.247	
Philadelphia	129	61.7	62.4	-0.7	0.938	
Pittsburgh	163	60.8	63.6	-2.8	0.728	
Portland	137	58.2	42.1	16.1 *	0.069	
Salem	133	57.4	60.4	-3.0	0.747	
San Jose	134	55.3	67.7	-12.4	0.168	
Sample size	2,024					

SOURCES: MDRC calculations from New Chance Enrollment Form and survey data.

NOTES: Calculations for this table used data for all 2,088 sample members for whom there were 18 months of follow-up survey data, including those with values of zero for outcomes and New Chance enrollees (i.e., experimentals) who did not participate in the program. The reported sample sizes may fall short of this number because of missing or unusable items from some sample members' questionnaires.

The percentages are adjusted using a two-way analysis of covariance procedure controlling for up to 36 kinds of difference in characteristics, other than site, before random assignment. The two categories used as factors were research status (i.e., membership in the experimental or control group) and site. Rounding may cause slight discrepancie[illegible]n sums and differences.

(a) A two-tailed t-test was applied to each regression-adjusted within-site impact. An F-test was applied to the interaction between sites and experimental or control status. The columns showing p-values are the statistical significance levels of each within-site impact or between-sites impact difference: That is, p is the probability that sample estimates are different from zero or from each other only because of random error. Statistical significance levels are indicated as *** = 1 percent; ** = 5 percent; * = 10 percent.

How One Program Delivered the Message on Repeat Pregnancies

The San Jose New Chance site is one of only two sites that saw a sizeable reduction in post-baseline pregnancies. After experiencing a rash of early program pregnancies, the New Chance coordinators devised a strategy to address the problem. In addition to strengthening the emphasis on family planning and life skills classes, the coordinators worked with staff to insure that the program delivered a clear, unambiguous message about the desirability of delaying subsequent pregnancies. All staff were expected to help deliver this message both in counseling individuals and in course content. For instance, during the employability development class participants might be asked how another pregnancy would affect their ability to undertake the particular course of study or occupation under discussion. Any staff who were uncomfortable discussing family planning were expected to refer participants to another staff person who was not. There were no baby showers nor other special attentions given to young women who were pregnant; if the conversation turned to a participant's pregnancy or new baby, staff interrupted and changed the subject. These actions combined to create an atmosphere that, over time, participants reinforced.

Moreover, MDRC staff noted that both the Denver and San Jose programs had implemented a family planning component that closely conformed to the New Chance guidelines. However, the relationships were less distinct with respect to the sites with unfavorable impacts. For example, enrollees in Portland gave above-average ratings on the degree to which pregnancy postponement was emphasized, and an above-average percentage at that site reported having received life skills classes. Thus, these analyses offer no clear-cut evidence as to why sites did or did not affect enrollees' efforts to postpone pregnancies.

D. Post-Baseline Pregnancies and Program Participation

On a descriptive level, it is possible to examine whether rates of subsequent pregnancy were associated with different levels of program participation within the experimental group. As indicated below, women who spent more time in New Chance activities were less likely to have had a post-baseline pregnancy:

	Enrollees Who Had a Post-Baseline Pregnancy (%)
Zero hours in New Chance	65.6
Bottom third (1 to 127 hours)	67.9
Middle third (128 to 378 hours)	56.8
Top third (more than 378 hours)	42.1

Pregnancy rates also declined in relation to the number of hours in the New Chance family planning classes:

	Enrollees Who Had a Post-Baseline Pregnancy (%)
Zero hours of family planning	66.5
Bottom third (1 to 3 hours)	57.5
Middle third (4 to 8 hours)	54.9
Top third (more than 8 hours)	46.4

This may mean that greater exposure to New Chance and to its family planning component helped young mothers to postpone a subsequent pregnancy, by affecting their motivation to have another baby in the short term or their contraceptive skills. Alternative explanations are, however, equally plausible. For example, women who became pregnant may have dropped out of the program earlier, thereby shortening the number of hours spent in any New Chance activity. Also, women who were highly motivated to pursue educational or occupational goals might have been especially likely to both spend many hours in the program and avoid another pregnancy.

E. Aggregate Impacts on Pregnancy Combined with Other Outcomes

Given the concern that another pregnancy could adversely affect the young mothers' ability to complete the New Chance program and move forward with their lives, it is useful to consider subsequent pregnancies in this sample in relation to GED attainment. For example, did program impacts on GED attainment occur only in conjunction with the avoidance of another pregnancy? Conversely, were impacts on subsequent pregnancies associated with failure to achieve a GED? Another important question concerns the relation between post-baseline pregnancies and living arrangements at follow-up. This section examines program impacts on pregnancies during the follow-up period conjoined with GED attainment and living arrangements.

Table 6.6 presents the results of these analyses. The first panel shows the percentage distribution of experimentals and controls for the four possible combinations of post-baseline (i.e., post-random assignment) pregnancy and GED attainment (i.e., did or did not have a pregnancy, crossed with did or did not have a GED or high school diploma). The table indicates that there were statistically significant program impacts for all four categories. Regardless of whether there was a post-baseline pregnancy, significantly more experimentals than controls had obtained a GED or diploma by follow-up: 21.3 percent of the experimentals versus 12.9 percent of the controls had both a pregnancy and a GED or diploma,[18] and 21.8 percent of the experimentals versus 17.1 percent of the controls had a GED or diploma but no post-baseline pregnancy. Another way to view these data is to consider only those in the first two rows: Among the women who had a post-baseline pregnancy, 37.6 percent of the experimentals, but only 24.5 percent of the controls, had achieved their GED by the time of the follow-up interview. Thus, it appears that avoiding a post-baseline pregnancy was much more strongly associated with school completion among the controls than among the experimentals.[19]

---

[18] Interestingly, among the experimentals with both GED completion and a post-baseline pregnancy, some 14.2 percent reported that the pregnancy was planned. In contrast, among the controls in the same category, 7.7 percent reported that the pregnancy was planned (not shown in tables).

[19] Among the women who had both a post-baseline pregnancy *and* a GED or diploma, slightly under two-thirds in each research group completed their schooling *before* the onset of the pregnancy, while roughly one-third received the education credential *after* the onset of the pregnancy (not shown in tables).

TABLE 6.6

IMPACTS OF NEW CHANCE ON PREGNANCY COMBINED WITH OTHER OUTCOMES AT 18 MONTHS AFTER RANDOM ASSIGNMENT

Outcome	Experimentals (%)	Controls (%)	Difference	p (a)
Joint percentage distribution of pregnancy during follow-up and educational attainment at 18 months (b)				
Had pregnancy, obtained GED or high school diploma (c)	21.3	12.9	8.5 ***	0.000
Had pregnancy, did not obtain GED or high school diploma	35.4	39.8	−4.5 **	0.037
No pregnancy, obtained GED or high school diploma (c)	21.8	17.1	4.7 ***	0.008
No pregnancy, did not obtain GED or high school diploma	21.5	30.2	−8.6 ***	0.000
	100.0	100.0		
Joint percentage distribution of pregnancy during follow-up and living arrangement at follow-up (b)				
Had pregnancy, living with partner/husband	16.0	12.9	3.1 *	0.058
Had pregnancy, not living with partner/husband	40.7	39.8	0.8	0.708
No pregnancy, living with partner/husband	9.0	9.5	−0.5	0.729
No pregnancy, not living with partner/husband	34.3	37.8	−3.5	0.109
	100.0	100.0		
Sample size	1,408	680		

SOURCES: MDRC calculations from New Chance Enrollment Form and survey data.

NOTES: Calculations for this table used data for all 2,088 sample members for whom there were 18 months of follow-up survey data, including those with values of zero for outcomes and New Chance enrollees (i.e., experimentals) who did not participate in the program.

The percentages are adjusted using linear analysis of covariance procedures controlling for up to 51 kinds of difference in characteristics before random assignment. Rounding may cause slight discrepancies in sums and differences.

(a) A two-tailed t-test was applied to each regression-adjusted difference between average experimental and control group outcomes. The column labeled "p" is the statistical significance level of the difference between experimental and control group outcomes: That is, p is the probability that average outcomes are different only because of random error. Statistical significance levels are indicated as *** = 1 percent; ** = 5 percent; * = 10 percent.

(b) Using a multinomial logit estimator, the distributions for experimentals and controls in these mutually exclusive categories were compared and found to be statistically significantly different from one another.

(c) Includes both women who earned a GED or high school diploma after random assignment and those who had the credential at random assignment.

Table 6.6 indicates that significantly more experimentals than controls were in the "best" joint outcome (achievement of the GED or diploma and no pregnancy). Conversely, significantly fewer experimentals (35.4 percent) than controls (39.8 percent) were in the least desirable (from the program's perspective) category: having a post-baseline pregnancy and not obtaining a GED or diploma. Nevertheless, it is worth noting that more young mothers in *each* research group were in this latter category than in any other.

The bottom panel of Table 6.6 presents the percentage distribution for the four possible categories resulting when post-baseline pregnancy status was conjoined with follow-up living arrangements (living with a partner or husband, or not). Significant impacts were confined to a single category for this outcome: Significantly more experimentals (16.0 percent) than controls (12.9 percent) had a post-baseline pregnancy *and* were living with a partner or husband at follow-up. Experimentals were no more likely than controls to have gotten pregnant during the follow-up period and *not* be living with a partner or husband. Since information on the dates that living arrangements began and ended was not obtained, it is impossible to determine whether the pregnancy preceded or followed the initiation of living with a partner or husband. For example, it is not possible to tell whether the occurrence of another pregnancy prompted women to move in with partners or husbands, or whether living with partners or husbands exposed the women to a higher risk of pregnancy. However, the findings do suggest that, among the experimentals, there was a somewhat greater likelihood that pregnancies would occur in the context of a relationship that was sufficiently stable to result in cohabitation.

F. Fertility in New Chance and Other Demonstrations

In the New Chance sample, the rate of pregnancies occurring after random assignment was high for both experimentals and controls, with a higher rate among the experimentals. Although the findings with regard to post-baseline pregnancy were inconsistent with the intent of the New Chance model, they were not inconsistent with findings from other research: Most programs for young mothers have found it extremely difficult to reduce the rate of subsequent pregnancies and births among women who gave birth as teenagers.

Some specific comparative information is presented in Table 6.7. This table presents rates of post-baseline pregnancy and birth from the evaluations of three other large-scale demonstration programs that served disadvantaged young mothers: the JOBSTART Demonstration, the Teenage Parent Demonstration, and Project Redirection. As noted in previous chapters, these interventions served somewhat different populations than that served in New Chance, but the most relevant subgroups of these programs have been selected for the purpose of presenting data for comparison (except in the case of the Teenage Parent Demonstration, for which comparable fertility data were available for the aggregate sample only).[20]

---

[20] Information on the rates of pregnancy and birth was not available for the Teenage Parent Demonstration subgroup of school dropouts who did not have a high school diploma or GED. However, the mean number of pregnancies at the time of the 28-month follow-up interview was similar for the overall Teenage Parent Demonstration sample and for the dropout subgroup: Among experimentals, the mean number of pregnancies was 1.0 for the dropout subgroup, compared to .98 for the overall sample; among controls, the means were .99 and .96, respectively.

TABLE 6.7

A COMPARISON OF FERTILITY–RELATED IMPACTS IN NEW CHANCE AND SELECTED OTHER PROGRAMS

Program and	Percent with a Pregnancy			Percent with a Birth		
Follow–Up Period	Experimentals	Controls	Difference	Experimentals	Controls	Difference
New Chance						
18 months	57.0	53.0	4.0 *	28.4	26.2	2.2
JOBSTART mothers (a)						
24 months	59.1	53.1	6.0	32.7	25.4	7.3 *
Teenage Parent Demonstration						
18 months (b)	48.4	45.6	2.8	27.8	26.3	1.5
24 months (c)	57.6	54.4	3.2	40.1	37.5	2.6
Project Redirection (dropout subsample)						
12 months (d)	23.0	34.0 (f)	−9.0 **	N/A	N/A (f)	N/A
24 months (e)	56.0	58.0 (f)	−2.0	32.0	41.0 (f)	−9.0

SOURCES: MDRC calculations from New Chance Enrollment Form and survey data; Cave and Doolittle, 1991; Maynard, Nicholson and Rangarajan, 1993; Polit, Kahn, and Stevens, 1985.

NOTES: N/A indicates that the specified data item was not available.

Statistical significance levels are indicated as *** = 1 percent; ** = 5 percent; * = 10 percent.

(a) The JOBSTART sample is made up of young mothers between the ages of 17 and 21 who did not have a high school diploma or GED, read below the eighth–grade level, lived with their own children, and were not enrolled in school at the time of sample enrollment.

(b) The aggregate rates presented here were calculated based on information on site–specific rates in the three demonstration sites; significance levels for the pooled sample are not available. However, at 18 months after random assignment, the experimentals had a significantly higher rate of pregnancy and birth in one site (Chicago). The 18–month rate in the Teenage Parent Demonstration sample possibly is lower than that in the New Chance sample because of differences in the eligibility criteria for the two studies; in particular, 12 percent of the Teenage Parent Demonstration sample was pregnant at random assignment.

(c) The aggregate rates presented here were calculated based on information on site–specific rates in the three demonstration sites; significance levels for the pooled sample are not available. However, at 24 months after random assignment, the experimentals had a significantly higher rate of pregnancy in two sites (Chicago and Newark), and a significantly higher rate of birth in one site (Chicago).

(d) The Project Redirection dropout subsample is made up of young mothers aged 17 or younger, most of whom were on welfare, who had dropped out of school.

(e) About 60 percent of the Project Redirection sample members were pregnant at baseline, which would depress the rates of subsequent pregnancy at follow–up.

(f) In Project Redirection, unlike the other demonstrations, the comparison group was not selected through random assignment.

Only one of the three comparison evaluations (the study of the Teenage Parent Demonstration) provides information about pregnancies and births at 18 months after baseline – i.e., the same follow-up time frame as in New Chance. Nevertheless, two things are clear from the information shown in Table 6.7. First, a majority of disadvantaged young mothers in all four programs had a subsequent pregnancy in two years or less of entering the research samples, and a sizeable minority had a subsequent birth. Second, none of these programs was effective in the long run in reducing subsequent pregnancies or births. In Project Redirection, there was a favorable impact on subsequent pregnancy at 12 months after baseline, but the difference was not significant at the 24-month point. Moreover, by five years after baseline, those in the experimental group had a significantly higher average number of births than those in the comparison group.[21] A significantly higher birthrate was also observed among the experimentals in the JOBSTART Demonstration at 24 months after random assignment. Also, births were significantly higher among the experimentals than among the controls in one of the three Teenage Parent Demonstration sites at both 18 and 24 months after random assignment, and pregnancies were higher among experimentals in two sites at the 24-month point (not shown in the table). Thus, closely spaced pregnancies and births are frequent among poor young mothers, and interventions have generally been unsuccessful in getting young mothers to postpone subsequent pregnancies and births.

In the New Chance sample, subsequent pregnancies were common regardless of the young women's initial characteristics. Table 6.4 shows that the rate of post-baseline pregnancies approached or exceeded 50 percent for nearly every subgroup in each research group, regardless of the women's initial abilities, family circumstances, or personal resources.[22] At the site level, even where experimental-control group differences looked most promising (Denver and San Jose), the rate of post-baseline pregnancies exceeded 40 percent for the experimental group. The consistently high rates of subsequent pregnancies across subgroups, sites, and demonstrations suggest that it is extremely difficult for staff in such programs to influence sexual and contraceptive behavior. For young, sexually active women, the task of avoiding a pregnancy typically must be managed on a daily basis, and it is a task that is subject to a wide range of interpersonal pressures over which program staff have no control.[23] Moreover, program staff are not always comfortable or skilled in dealing with family planning issues. Thus, there appear to be a number of reasons – programmatic and other – for programs to have difficulty in lowering the rate of subsequent pregnancy among teenage mothers. However, this does not explain why programs have been found to *increase* the rate of subsequent pregnancy or birth in this population.

---

[21] A subsample of the Project Redirection sample was followed up at five years after baseline. In this subsample, the total number of births was 2.4 for the experimentals and 2.0 for comparison group members ($p < .05$).

[22] Indeed, in a regression analysis attempting to predict the occurrence of a post-baseline pregnancy on the basis of baseline characteristics, the power of the prediction was extremely low ($R^2 = .04$). The best predictor was whether the young mother said at baseline that she wanted no more children. Another significant predictor, however, was number of children already born: The more children a woman had at baseline, the more likely she was to have a pregnancy during the follow-up period (not shown in tables).

[23] Program staff have noted that the young women often get pregnant when they get a new boyfriend, either because of direct partner pressure or as a means of cementing the relationship.

The New Chance results suggest that the factors affecting contraceptive vigilance, pregnancy planning, and pregnancy termination decisions in this population are complex, and that different forces may be operating on different sets of women. Some of the experimental group women may have felt disappointed because their circumstances had not changed as much as they had hoped for when they enrolled in New Chance, or stressed because transitions to new environments (e.g., the workplace or college) may have been intimidating – or because they may have feared success. Among these women, there may have been considerable ambivalence about another pregnancy – ambivalence that could have led to inconsistent contraception and to a variety of decisions regarding the resolution of the pregnancy.

Another possibility, perhaps more relevant to other enrollees, is that young mothers in self-sufficiency-oriented programs may feel that it is easier to have another baby – an area where many feel that they are successful – than to pursue activities where success may be harder to achieve. The pregnancy impact could also partially reflect the fact that some young women who attained their GEDs – and experimentals had a higher rate of GED attainment than controls – felt that it was advantageous to complete their childbearing before moving on to further schooling or employment. In the New Chance sample, the fact that experimentals who were living with partners or husbands were especially likely to become pregnant is consistent with the interpretation that some of the young women felt that the circumstances were right to have another baby.

In summary, it is clear that programs for young mothers face a very difficult challenge in helping them to postpone pregnancies. And while it appears that New Chance affected the circumstances of the young mothers' lives in such a way that decisions about the timing of pregnancy were influenced, it is not at all clear what the effects of accelerated childbearing might be in this population in the long run. It is especially noteworthy that many women in the experimental group obtained a GED despite having had a subsequent pregnancy. It remains for the 42-month data to reveal what effects, if any, childbearing patterns will eventually have on employment and welfare receipt.

IV. Impacts on Health-Related Outcomes

Although there is an extensive literature on the relationship between maternal age at first birth and infant health, few studies have examined the health outcomes of adolescent mothers themselves beyond the immediate postnatal period. However, even in the absence of specific research on the health of teenage mothers, it seems safe to assume that the young mothers in the New Chance sample were at higher-than-average risk of adverse health outcomes, given the strong link between poverty, on the one hand, and health problems and health risks, on the other (Hughes et al., 1989; Meyers et al., 1983; Gladstein, Rusonis, and Heald, 1992; Schoenborn, 1986; Yamaguchi and Kandel, 1984).

New Chance sought to improve the health of program participants (and their children) through health education classes and linkages to community health facilities; at a few sites, health care services were available on-site at the programs. The health component was designed to promote positive health practices and to address specific health problems that might interfere with regular program participation. This section examines program impacts on health-related outcomes measured in the 18-month survey.[24]

Table 6.8 presents information on several health-related outcomes for the aggregate sample. This table suggests that, despite the young age of the women, health problems were not uncommon. About half of the women in the experimental and control groups rated their health at follow-up as "excellent" or "very good," but the other half gave themselves a lower health rating (13.8 percent said that their health was "fair," and another 1.9 percent said that it was "poor"). The ratings in the experimental and control groups were very similar and were not significantly different.

The young mothers in the New Chance sample reported that they had spent an average of four to five days in bed because of illness in the 18 months since random assignment (5.5 for experimentals and 4.2 for controls). Some 12.7 percent of the controls and 16.3 percent of the experimentals had been hospitalized at least once over the 18-month follow-up period, excluding hospitalizations for childbirth.[25] For both of these health indicators, the group differences favoring controls were not significant.

The majority of mothers in both groups were covered by Medicaid (Medi-Cal in California) or, less commonly, private health insurance (about 11 percent of the sample). However, about one out of every 12 young women in the sample had no health insurance. Health care coverage was similar in the two groups.

Since the issue of STDs was discussed in New Chance health classes, the follow-up interviews asked whether the young women had had an STD (specifically gonorrhea, syphilis, genital herpes, chlamydia, or AIDS) in the 12-month period prior to the interview. Incidence of any STD was reported by about 10 percent of the sample.[26] The somewhat higher (but not significantly higher) percentage of experimentals (10.3 percent) than controls (8.5 percent) reporting an STD could have reflected the participants' greater awareness of STDs and therefore a slightly greater likelihood that a disease would have been detected and diagnosed.

Questions on alcohol and drug use were also included in the 18-month survey, because the program health classes covered these topics and also because program staff at several sites attributed

---

[24] It should be noted that the health measures used in the survey were extremely limited and did not adequately capture the full range of topics covered in health education classes. (The survey included several standard and quite general health measures, which in some cases were not linked to specific areas covered in the program.) Moreover, all of the health outcomes were based on self-reports and did not include objective and sensitive physiological measures of health status (e.g., objective measures of drug use, nutritional status, and obesity, etc.)

[25] The number of days in bed because of illness ranged from zero (43.8 percent of the sample) to 300. Among those reporting any days in bed, the average was nine days. Among those who had ever been hospitalized since random assignment, the number of hospitalizations ranged from one to 32, with an average of 1.9 different hospitalizations.

[26] The most commonly reported STD was chlamydia, acknowledged by 6.3 percent of the sample. Four sample members reported that they had AIDS.

TABLE 6.8

IMPACTS OF NEW CHANCE ON HEALTH-RELATED OUTCOMES AT OR WITHIN 18 MONTHS AFTER RANDOM ASSIGNMENT

Outcome	Experimentals	Controls	Difference	p (a)
Personal health rating at follow-up (%)				
"Excellent" or "very good"	53.6	54.2	-0.6	0.795
"Good," "fair," or "poor"	46.3	45.3	1.0	
Average number of days in bed more than half a day due to illness or injury during follow-up (b)	5.5	4.2	1.3	0.266
Hospitalized at least once during follow-up (excluding hospitalizations for childbirth) (b) (%)	16.3	12.7	3.6	0.136
Had no Medicaid/Medi-Cal or private insurance at follow-up (%)	7.9	9.4	-1.5	0.254
Had a sexually transmitted disease in prior 12 months (c) (%)	10.3	8.5	1.8	0.198
Drank enough alcohol to feel high at least once in prior month (%)	37.4	39.2	-1.8	0.425
Used drugs at least once in prior month (d) (%)	14.1	13.1	1.0	0.531
Sample size	1,366	658		

SOURCES: MDRC calculations from New Chance Enrollment Form and survey data.

NOTES: Calculations for this table used data for all 2,088 sample members for whom there were 18 months of follow-up survey data, including those with values of zero for outcomes and New Chance enrollees (i.e., experimentals) who did not participate in the program. The reported sample sizes may fall short of this number because of missing or unusable items from some sample members' questionnaires.

The averages or percentages are adjusted using linear analysis of covariance procedures controlling for up to 51 kinds of difference in characteristics before random assignment. Rounding may cause slight discrepancies in sums and differences.

Distributions may not total 100.0 percent because of rounding.

(a) A two-tailed t-test was applied to each regression-adjusted difference between average experimental and control group outcomes. The column labeled "p" is the statistical significance level of the difference between experimental and control group outcomes: That is, p is the probability that average outcomes are different only because of random error. Statistical significance levels are indicated as *** = 1 percent; ** = 5 percent; * = 10 percent.

(b) These questions were asked of half the research sample, selected at random.

(c) Includes gonorrhea, syphyllis, herpes, chlamydia, and AIDS.

(d) Includes marijuana, cocaine, crack, heroin, PCP, and ice.

some attendance problems to drug and alcohol abuse. As Table 6.8 shows, the percentage of experimentals (37.4 percent) and controls (39.2 percent) who said that they had gotten high on alcohol in the prior month was not significantly different.[27] Among those who reported getting high on alcohol in the prior month, the majority (68.1 percent) said that it had happened only once or twice, while the others said that it had happened three to five times (22.0 percent), six to 10 times (5.2 percent), or more than 10 times (4.7 percent).

The experimental and control groups were also similar with respect to reported drug use. About one out of every eight women admitted to using drugs in the prior month. Marijuana was the most commonly used drug, reported by 13.3 percent of the sample. Use of cocaine or crack was reported by 1.5 percent of the women; 0.4 percent of the sample reported that they had used heroin, ice, or PCP. Among those reporting any drug use, about 10 percent acknowledged that they were habitual users – i.e., had used drugs on at least 10 days the previous month.

In summary, the experimental and control groups did not differ significantly with respect to any of the indicators of health and health care included in the survey. This may mean that health behaviors and health outcomes are relatively difficult to influence in the context of an intervention not specifically aimed at improving health. However, it could also reflect the limitations of self-reports for measuring health variables, and the inclusion of a fairly restricted number of health questions on the survey.

V. Impacts on Emotional Well-Being

Poor young mothers have been found to suffer from various emotional problems, such as poor self-esteem, high levels of depression and stress, inadequate social support and coping skills, and low feelings of self-efficacy (Colletta, 1983; Ketterlinus, Lamb, and Nitz, 1991; Musick, 1991). And, indeed, the New Chance Enrollment Forms documented that about half of the program applicants had a score on a widely used scale that indicates they were at risk of clinical depression (see Table 2.1).

New Chance programs were specifically structured to foster positive emotional growth among the participants. Staff deliberately sought to bolster participants' self-esteem and to offer them a warm and supportive (but demanding) environment.[28] The goal of improving the emotional well-being of participants was consistent with the considerable evidence indicating that high levels of depression and stress constitute important barriers to effective functioning in adult roles, particularly with regard to parenting (e.g., Lyons-Ruth, Connell, and Grunebaum, 1990; Richters and Pellegrini, 1989; Simons et al., 1993; Cooley and Unger, 1991). This section reviews the evidence with regard to the program's effects on the emotional well-being of young mothers.

---

[27] However, there were significant experimental-control group differences for several subgroups. For example, among white sample members, 29.9 percent of the experimentals compared to 39.4 percent of the controls reported having gotten high on alcohol in the prior month ($p < .05$). There were also significant program impacts favoring the experimentals for the following subgroups: women who were 16 or younger when they had their first child; women who were living with their mothers at baseline; and women whose baseline reading scores were at the eighth- or ninth-grade level.

[28] Some sites added mental health specialists to their staff to address the emotional problems experienced by numerous participants.

The follow-up interviews included the widely used Center for Epidemiological Studies Depression (CES-D) Scale as a major indicator of emotional well-being (Radloff, 1977).[29] Scores on this scale can theoretically range from 0 (not at all depressed) to 60 (severely depressed). Scores of 16 or higher are considered to be indicative of a level of depression that places the person at risk of clinical depression.[30] As shown in Table 6.9, the mean score for both experimentals and controls was about 16, suggesting a fairly high level of depression for the aggregate sample. Nearly half of the women in each group (about 45 percent) obtained a score of 16 or higher at follow-up. The difference between the scores of experimentals and controls on the follow-up CES-D scale was not significant.[31]

When changes between baseline and follow-up CES-D scores for individual sample members are examined, it is clear that fairly wide fluctuations over time are not unusual in this population. The changes in CES-D scores ranged from minus 44 (i.e., greatly improved de-pression scores) to plus 44 (i.e., much worse depression scores). The average change was -2.0 – i.e., the young mothers became, on average, somewhat less depressed over the 18-month follow-up period. This average masks considerable change in both directions for many young mothers: Nearly 40 percent of the sample had a score change of 10 or more points on the depression scale, indicating that some fairly sizeable changes in these women's emotional well-being had occurred.[32] The average score change was somewhat greater among women in the control group (mean of -2.5) than among those in the experimental group (mean of -1.8), and this difference was nearly statistically significant. In other words, although the two groups had similar levels of depression at follow-up, the control group's level of depression had improved somewhat more over time than that of the experimental group.

---

[29] The CES-D consists of 20 statements such as "I had crying spells" and "I felt depressed." Respondents indicated how often in the past week the statement was true for them. The CES-D has been used in many studies of disadvantaged women (Belle, 1982; Colletta, 1983; Vega et al., 1986), and has been shown to have excellent validity and reliability. In the present sample, the internal consistency reliability for the scale was quite good (.88.) Actual scores ranged from 0 to 57.

[30] In Radloff's (1977) validation studies, 70 percent of a psychiatric inpatient sample had scores of 16 or above. In another patient group, the average CES-D score at admission was 39.1, with no score falling below 16. On the other hand, in a general population, only 21 percent had a score of 16 or higher.

[31] At the subgroup level, none of the group differences in follow-up CES-D scores favored experimentals at significant levels. However, controls had significantly more favorable CES-D scores in five subgroups: women who were living with their mothers at baseline; those with only one child at baseline; those not on welfare at baseline; those who earned $500 or less in the year before random assignment; and those whose families were sometimes on welfare when they were growing up. At the site level, there was a significant program impact favoring experimentals in Chula Vista, but an unfavorable impact in Detroit.

[32] Two of the outcomes examined in this chapter (living arrangements and post-baseline pregnancies) appear to have been associated with changes in CES-D scores. Women who were living with a parent or grandparent at follow-up had significantly *less* improved depression scores (mean = -1.5) than women who were not living with an adult relative (mean = -2.2). Women who had a post-baseline pregnancy (mean = -1.6) had less improved scores than those who did not have another pregnancy (mean = -2.4). It is, of course, impossible to know whether changes in levels of depression affected decisions about living arrangements and pregnancy, or vice versa – or whether both had other determinants in common.

TABLE 6.9

IMPACTS OF NEW CHANCE ON INDICATORS OF EMOTIONAL WELL-BEING AT 18 MONTHS AFTER RANDOM ASSIGNMENT

Outcome	Experimentals	Controls	Difference	p (a)
Average score on the CES-D (depression) Scale (b)	16.3	15.7	0.6	0.188
At risk of clinical depression (b) (%)	45.2	44.2	0.9	0.685
Change in CES-D (depression) Scale from random assignment to follow-up (c)	-1.8	-2.5	0.7	0.116
Average score on Mastery (self-efficacy) Scale (d)	22.1	22.2	-0.1	0.483
Average score on Difficult Life Circumstances Scale (e)	2.8	2.7	0.1	0.230
Average number of sources of social support cited as available	1.9	1.9	0.0	0.406
Reported no one available as a social support (%)	5.4	8.1	-2.7 **	0.017
Average level of satisfaction with available social support (f)	8.2	7.9	0.3 ***	0.009
Sample size	1,366	658		

SOURCES: MDRC calculations from New Chance Enrollment Form and survey data.

NOTES: Calculations for this table used data for all 2,088 sample members for whom there were 18 months of follow-up survey data, including those with values of zero for outcomes and New Chance enrollees (i.e., experimentals) who did not participate in the program. The reported sample sizes may fall short of this number because of missing or unusable items from some sample members' questionnaires.

The averages or percentages are adjusted using linear analysis of covariance procedures controlling for up to 51 kinds of difference in characteristics before random assignment. Rounding may cause slight discrepancies in sums and differences.

Distributions may not total 100.0 percent because of rounding.

(a) A two-tailed t-test was applied to each regression-adjusted difference between average experimental and control group outcomes. The column labeled "p" is the statistical significance level of the difference between experimental and control group outcomes: That is, p is the probability that average outcomes are different only because of random error. Statistical significance levels are indicated as *** = 1 percent; ** = 5 percent; * = 10 percent.

(b) Those with scores below 16 on the CES-D are considered not to be at risk of depression; those with scores of 16 and above are considered at risk.

(c) The values here reflect follow-up CES-D scores minus baseline CES-D scores. Negative values indicate improvement - i.e., less depression at follow-up.

(d) The Mastery Scale measures sense of mastery over personal events. Scores can range from 7 to 28.

(e) The scores are the total numbers of ongoing problems or stresses the respondent faces, of a list of 10 problems.

(f) Satisfaction with social support was rated on a scale from 0 ("extremely dissatisfied") to 10 ("extremely satisfied").

The Mastery Scale (Pearlin et al., 1981) is a seven-item scale designed to measure a person's sense of self-efficacy or mastery over external events.[33] Scores on this scale can theoretically range from 7 (low perceived self-efficacy) to 28 (high self-efficacy). For both the experimentals and controls, the mean Mastery Scale score was just over 22, suggesting fairly positive perceptions of self-efficacy. The small group differences were not significant.

The follow-up interview also included a measure of stress, the Difficult Life Circumstances (DLC) Scale. Unlike many other measures of stress, which focus on life changes over a fixed period of time, the DLC scale was designed to measure ongoing or habitual stress that is often a feature of living in disadvantaged circumstances. The 10-item scale used in this study was adapted from a scale by Booth and colleagues (1989).[34] As shown in Table 6.9, the average number of reported difficult life circumstances was 2.8 for the experimental group and 2.7 for the control group, a difference that was not significant.[35] Ninety-one percent of the sample reported one or more of the 10 listed problems. The most commonly reported problem, cited by 45 percent of the sample, was trouble finding a good place to live.

Several brief measures of social support were also included in the follow-up interviews. Respondents were asked to whom they could turn for moral support when they had a problem or just needed to talk. On average, women in both groups cited 1.9 different types of people on whom they could count for support. Mothers were most frequently mentioned (47.2 percent), followed by female friends (38.5 percent), partners or husbands (35.6 percent), and sisters (28.7 percent). Some 5.4 percent of the women in the experimental group, compared to 8.1 percent of the controls, said that they had *no one* available as a social support, a difference that was statistically significant.[36] This presumably reflected the fact that women in the experimental group had greater opportunity to meet supportive people, although it might also have reflected enhanced skills in communication and managing interpersonal relationships. Respondents also rated their degree of satisfaction with the social support available to them on a scale from 0 (extremely dissatisfied) to 10 (extremely satisfied). As indicated in Table 6.9, women in the experimental group gave significantly higher average ratings of satisfaction with available social support than those in the control group (8.2 versus 7.9, respectively).[37]

---

[33] An example of an item from the Mastery Scale is: "I have little control over the things that happen to me." In the present study, the internal consistency reliability of this scale was acceptable (.70). Scores spanned the full theoretical range, from a low of 7 to a high of 28.

[34] Respondents were asked whether they were experiencing 10 specific problems. Examples of items are: "Do you have a relative or boyfriend who is in jail?" and "Have you been robbed, mugged or attacked in the past year?" Total scores, calculated by summing the number of "yes" responses, ranged from 0 to 9 in this sample. Standard internal consistency reliabilities were not considered appropriate for this scale, because there is no reason to expect that a person with one particular problem would also have another particular problem.

[35] Women with a post-baseline pregnancy, however, reported significantly more difficult circumstances than women who avoided one (mean of 2.9 versus 2.5, respectively). This difference was accounted for primarily by three items on the DLC scale. Those with a pregnancy were significantly more likely than those without one to say that they had daily arguments with their partners, that they were having problems with a former partner or husband, and that they were having trouble finding a good place to live.

[36] The impact on this outcome, while modest, was quite consistent across subgroups and sites. The experimentals were less likely than the controls to say that they had no social support in virtually every subgroup examined, and in 16 out of 36 subgroup comparisons, the difference was statistically significant. The group difference favored experimentals in 13 of the 16 sites, though only at one site (Lexington) was the difference significant.

Thus, the effects of New Chance on indicators of emotional well-being appear to have been mixed. Although the level of depression at follow-up was similar in the two groups, women in the control group had improved their depression scores somewhat more than those in the experimental group. This might have reflected a number of differences in the follow-up circumstances of the two groups, such as a higher incidence of new pregnancies among experimental group women. It could also have reflected a higher level of stress among the experimentals, who might have been faced with conflicting demands as a result of program participation, and perhaps higher – and unfulfilled – expectations that their situations would improve. On the other hand, controls were significantly more likely to have had no available social supports, and were generally less satisfied with the support available to them. This is noteworthy because inadequate social support has been found to be related to a variety of personal and emotional problems (such as poor health, alcohol and drug use, inadequate parenting, etc.). However, the absolute magnitude of differences was rather small.

---

[37] New Chance programs also made efforts to improve the life skills and citizenship of participants – i.e., to encourage these young women to more fully participate in activities that are considered the norm in our society such as voting or driving. In brief, an examination of impacts in this area revealed that a significantly higher percentage of experimentals (52.9 percent) than controls (47.1 percent) were registered to vote, although a comparable percentage (about 17 percent) had actually voted in the last general election, which was in all cases prior to the Presidential election in November 1992. The impact on voter registration was attributable mainly to large group differences in Allentown (48.4 percent of the experimentals versus 13.6 percent of the controls) and Lexington (23.4 percent of the experimentals versus 1.5 percent of the controls). With respect to having a driver's license, the small group difference favoring experimentals (39.0 percent versus 36.7 percent) was not significant, although the difference was significantly favorable to experimentals in Pittsburgh. More of the women in the experimental group (21.1 percent) than in the control group (17.1 percent) were without a home phone 18 months after random assignment, a difference that was nearly significant. This difference may have been tied to the fact that fewer experimentals than controls were living with an adult relative.

CHAPTER 7

IMPACTS ON PARENTING, CHILD CARE, AND CHILD HEALTH

I. Introduction

A. Background

As indicated in Chapter 1, New Chance had an explicit two-generational focus; i.e., it endeavored to improve outcomes for children as well as for their mothers. The program sought to affect child development outcomes both directly, through services to the children, and indirectly, through services to the participating mothers.

The most direct path was through the provision of developmentally appropriate child care while the mothers participated in program activities. More than half of the New Chance programs had on-site child care centers that were encouraged to adhere to guidelines developed in collaboration with a group of child development experts. The provision of high-quality child care was explicitly included in the New Chance model because such care has been found to have beneficial effects on the development of disadvantaged children (Burchinal, Lee, and Ramey, 1989; Martin, Ramey, and Ramey, 1990; Phillips, McCartney, and Scarr, 1987).

The second direct path involved the provision of pediatric care, usually through referral to health care providers. Pediatric services for participants' children were intended to enhance the health and health care utilization of participants' children who, because of their poverty, are at higher-than-average risk of numerous medical problems.

The third path was through the provision of parenting instruction and support. The parenting component of New Chance sought both to strengthen the parenting skills of participants (e.g., by helping them to better understand developmental phases of childhood and effective methods of disciplining children) and to support and encourage participants to better cope with the stresses of parenthood. As was the case with the child care component, child development experts collaborated in the preparation of guidelines for the parenting component of the program. Good parenting skills have repeatedly been found to be predictors of children's positive social and emotional development (Denham, Renwick, and Holt, 1991; Pianta, Sroufe, and Egeland, 1989) as well as cognitive and school performance outcomes (Coates and Lewis, 1984; Estrada et al., 1987; McGowan and Johnson, 1984). Moreover, there is evidence from Project Redirection that comprehensive programs that offer parenting education to disadvantaged young mothers can have positive effects on the quality of the home environment and on children's development (Polit, Quint, and Riccio, 1988).

Additionally, as suggested in the overall model presented in Figure 2.1, there were reasons to hypothesize that improved child development outcomes would be observed among children of experimental group members as a result of several indirect effects. In particular, it was expected that if New Chance improved the educational attainment and financial circumstances of the mothers, the children would be among the primary beneficiaries. There is considerable evidence that a higher level of maternal education and higher family income are associated with better cognitive and social development in children (Garcia-Coll, 1990; Garrett et al., 1994; Patterson, Kupersmidt, and Vaden,

240

1990; Takeuchi, Williams, and Adair, 1991). It was also expected that if the program had a positive effect on the mother's emotional well-being and psychological resources, the quality of the mother-child relationship would be enhanced: Mental health variables repeatedly have been found to be related to mother-child relations and to children's social and emotional development in poor and minority families (McLoyd, 1990; Simons et al., 1993).

In summary, New Chance was explicitly designed to have beneficial effects on the development of young children, and therefore it was important to evaluate whether such effects occurred. However, direct measures of developmental progress were not included in the 18-month interview, largely because of the difficulty of measuring child development outcomes for children under age 3 in the context of a survey interview. (Measures of cognitive and socioemotional development of the children *are* included in the 42-month follow-up interview and will be examined in the final report.)

The 18-month interview did, however, include questions on aspects of child-rearing known to have important effects on child outcomes. This chapter presents the results. Specifically, it examines whether participation in New Chance affected various aspects of parenting, including parenting attitudes, parental stress, and the overall quality of the home environment. Additionally, the chapter examines child care use since baseline (i.e., since random assignment), an area of great policy relevance. Finally, impacts on child health outcomes are discussed.

B. The Focal Child

Because of resource constraints, it was not possible to examine program impacts on all of the children of the sample members. Instead, one of each sample member's children was identified as the "focal child" – i.e., the child who would be the focus of all survey questions relating to a specific child. In scheduling the 18-month survey interview, interviewers asked mothers to have the focal child present. In this chapter, all references to the sample members' children concern the focal child, unless otherwise stated.

The majority of women in the sample (64.9 percent) had only one child at baseline, and this child automatically was considered to be the focal child. When there were two or more children at baseline, the focal child was randomly selected from among them.[1] Information about a focal child was obtained for 94.7 percent of the women who completed a follow-up interview.

At the time of the 18-month interview, the focal children ranged in age from 18 months to 8 years; their average age was 3.2 years. The majority of these children (81.6 percent) were first-borns, consistent with the fact that most young women had only one child at baseline. There were slightly more male than female focal children in the sample (52.9 percent versus 47.1 percent, respectively). About 12 percent of the focal children were living in the same households as their biological fathers. Among the children not living with their fathers, one-third (33.7 percent) saw their fathers at least once a week, but nearly another third (27.4 percent) had not seen their fathers at all in the previous

---

[1] If the randomly selected focal child was not living with the mother at the time of the follow-up interview or was otherwise unable to participate in the interview, the interviewer was allowed to substitute another child as the focal child – but only if the second child was one who had been born prior to baseline and was currently living with the mother. Substitutions occurred for about 0.4 percent of the sample.

12 months, according to the mothers' reports. The majority of children not living with their fathers had received no financial child support from their fathers in the year prior to the interview (66.8 percent), and about half (47.8 percent) had never received monetary support.

C. A Preview of the Findings

The findings in this chapter indicate that the focal children of experimental group members were exposed to different experiences than their control group counterparts in the 18-month period since random assignment, particularly with respect to child care. Even though the majority of children of control group members had been in a child care arrangement during the follow-up period, they were likely to have been in care for shorter periods of time and were primarily cared for by relatives. Children of experimentals were substantially more likely to have been cared for in day care centers or preschools; they were also more likely to have entered regular non-maternal child care before age 1. Impacts on the use of child care arrangements were significant across most subgroups and sites, but the effects were especially powerful among the most disadvantaged subgroups of women.

The children of experimentals were also being raised in home environments where there was more emotional support, and by mothers who expressed less authoritarian parenting attitudes. However, on both of these indicators, the group differences, while statistically significant, were small. Moreover, the overall quality of the home environment was similar in both groups, as was the level of parenting stress.

Taken together, the group differences with regard to child care and parenting measures at the 18-month point do not support a high level of optimism for sizeable child development impacts in the long run. The differences in the parenting and home environment measures were small, and differences in child care arrangements, while substantial, had disappeared by the time of the follow-up interview. However, child care impacts were particularly strong among subgroups of the most disadvantaged women (e.g., those with very low baseline reading scores and those with high baseline depression scores), so differences in child development outcomes may eventually emerge among these subgroups.

Finally, the program had no effect on the indicators of child health measured in the survey (e.g., injuries, hospitalizations, and sick days since random assignment). However, mothers in the experimental group were somewhat more likely to say that they had a particular place they went to for health care for the focal child, suggesting that the programs were helpful in making health care linkages for the participants and their children.

II. Impacts on Parenting and the Home Environment

As indicated in Chapter 1, research has fairly consistently shown that teenage mothers are less competent parents and tend to raise their children in less favorable home environments than women who delay childbearing. These findings have important implications for the children of young mothers, who have been found to have less promising developmental outcomes than other children (Moore and Snyder, 1991; Brooks-Gunn and Furstenberg, 1986; Cooley and Unger, 1991; Kinard and Klerman, 1983). Since poverty is also associated with children's cognitive and behavioral problems, children born to young women who are poor, like those in the New Chance research sample, are especially at risk of developmental difficulties.

The parenting component in New Chance was designed to promote positive parenting practices, to foster the development of healthy mother-child relationships, and to reduce the stresses associated with parenthood – with the primary ultimate aim of stimulating the early development of the participants' children. The program's effects on parenting and the home environment were assessed at 18 months after random assignment.

The measures used in the 18-month interview were based largely on what the mothers themselves reported. Although maternal self-reports are not ideally suited to the measurement of parenting behaviors, in-depth observation of mother-child interactions by child development experts was not a viable alternative for the entire sample.[2]

In the 18-month interview, the quality of the overall home environment was measured with a shortened version of the Home Observation for Measurement of the Environment (HOME) Scale (Caldwell and Bradley, 1984). The HOME, a widely used measure of home environmental processes that have been shown to be related to children's development, relies heavily on the mothers' reports but also includes several items that call on the interviewer to record observations of her own. The interviewers' observations primarily concerned ratings of the quality of the physical environment (e.g., its cleanliness and safety) and specific aspects of mother-child interactions (e.g., whether the mother spanked the focal child during the interview). Three additional self-report parenting scales were included in the survey: a Parenting Stress Scale, a Maternal Warmth/Responsiveness Scale, and a Maternal Control/Punitiveness Scale. Together with the dimensions tapped by the HOME, these scales collectively measure aspects of mother-child relationships that child development experts regard as critical.

The remainder of this section examines whether participation in the New Chance program had positive effects on these various measures of parenting and the home environment. As in other chapters, impacts for the sample as a whole (i.e., aggregate impacts) are examined first. Then impacts for selected subgroups are considered. The relationship between HOME scores and levels of participation in New Chance is also scrutinized.

A. Aggregate Impacts on Parenting and the Home Environment

1. Impacts on the Home Environment. The HOME scale used in the 18-month interviews was the version of the HOME used in the National Longitudinal Survey of Youth, or NLSY (Baker and Mott, 1989), referred to as the HOME-SF (short form). For the purpose of the impact analyses, adaptations to the HOME-SF were made, but the scale was also scored according to the NLSY procedures so that the home environments of the New Chance sample could be compared to those of a national sample.[3] In 1986, the HOME-SF was used in the NLSY for the nearly 5,000 children of a national sample of women who had been born between 1958 and 1965. Young mothers are disproportionately represented in the 1986 NLSY survey, so that the *children* are not a sample representative of young children; nevertheless they constituted an important comparison group, particularly for children of young mothers. In this regard, it is notable that the home environments of the New Chance sample (with experimentals and controls combined) compared favorably with those of poorly educated women in the NLSY, and were not appreciably different from those of the aggregate NLSY sample.[4]

---

[2] However, a supplementary observational study has been conducted with a subsample of about 300 New Chance sample members. This study involved the videotaping of the mother and child during 30 to 40 minutes of interaction, and subsequent viewing and coding of the videotapes by two teams of child development experts. A monograph on this study, which will be available in 1995, will present an analysis of the extent to which the measures used in the 18-month survey are adequate proxies for measures included in the observational study.

In the version of the HOME used for the New Chance impact analyses, a total HOME scale and four subscales were created:

- **Cognitive Stimulation:** whether the home environment included cognitively stimulating resources – such as books and toys – and whether adults in the home engaged the child in stimulating activities, such as reading to the child.

- **Emotional Support:** whether the mother's interactions with the child were characterized by warmth and supportiveness.

- **Physical Environment:** whether the interior and exterior of the home were clean, safe, and pleasant.

- **Harsh Discipline:** whether the mother used physical or harsh methods to punish the child.

The HOME has three alternative forms, corresponding to the age of the child.[5]

---

[3] Adaptations to the HOME scale were made after it was determined that the NLSY scoring of the scale yielded very low reliability coefficients. Presumably, the much lower reliabilities in New Chance than were reported for the NLSY sample reflect the greater homogeneity of the New Chance sample, which makes discrimination between individuals more difficult to achieve. The adaptations made for this study involved adding several additional items to the HOME and using a trichotomous scoring procedure that made finer discriminations than the traditional dichotomous scoring of the HOME.

[4] Among children under the age of 3, the mean score on the HOME-SF in the 1986 NLSY sample was 13.4 for the aggregate sample and 12.5 for women with less than 12 years of education; among same-aged children in the New Chance sample, the mean was 13.7. Among children between the ages of 3 and 5, the mean HOME-SF score was 18.9 for the entire NLSY sample (17.6 for women with less than 12 years of schooling) and 18.3 for the New Chance sample. It is somewhat surprising that the New Chance families did so well on the HOME, given their level of disadvantage. This could possibly reflect the fact that many teenage mothers today have had some parenting education, while the same was less likely to have been true during the early 1980s. It might also mean that NLSY interviewers, who interviewed in both poor and middle-class homes, were more critical in their observations of poor homes than New Chance interviewers because they had more affluent homes with which to compare them.

[5] To analyze impacts on the HOME scores for the entire New Chance sample (rather than having to conduct separate analyses for children of different ages), the raw HOME scores were converted to standard scores that have a mean of 100 and a standard deviation of 15. Standard scores were age-standardized within one-year intervals.

Table 7.1 presents impact information on the subscale and total HOME scores for the full New Chance sample. The average total HOME scores were virtually identical (just over 100.0) for the experimental and control groups, reflecting similar home environments, overall, for the two groups' children. The experimental and control groups also had comparable average scores (all near 100) on three of the HOME subscales – i.e., the Cognitive Stimulation, Physical Environment, and Harsh Discipline subscales.

However, on the Emotional Support subscale of the HOME, the experimental group had higher mean scores (100.6) than the control group (99.3). Although the magnitude of the group difference was small, the fact that statistically significant group differences were observed on this subscale is noteworthy. The Emotional Support subscale, unlike the other HOME subscales, consisted almost exclusively of interviewer observations rather than maternal reports. For example, interviewers indicated whether the mother's voice conveyed positive feelings about the focal child; whether she caressed, hugged, or kissed the child at least once during the interview; whether the mother responded verbally to the child's speech during the interview; and whether she spontaneously praised the child's qualities or behavior during the visit. Thus, the experimental and control groups differed with respect to a subscale that was less likely than other subscales to be influenced by social desirability and other reporting biases.[6] It also can be argued that the Emotional Support subscale more directly reflects the emotional quality of the parent-child relationship than other HOME subscales. Nevertheless, despite the impact on an important subscale of the HOME, it must be acknowledged that the magnitude of the group differences on the Emotional Support subscale was quite small.[7] Moreover, the absence of group differences on the total HOME scale score is discouraging.

2. Other Parenting Measures. Table 7.1 also shows the program's impacts on the other parenting measures. The three additional parenting scales were developed specifically for this study, but most items were adapted from several existing scales, such as Abidin's Parenting Stress Index and the Block and Block Child-Rearing Practices Report.[8]

The Maternal Warmth/Responsiveness Scale is a three-item self-report scale intended to tap the mother's emotional warmth and nurturance toward the focal child, and her readiness to respond to the

---

[6] The risk of interviewer bias seems minimal. Interviewers knew whether respondents were in the experimental or control group, but they knew little about the actual intervention or the program goals. Interviewers also had no contact with program staff, nor were any interviews conducted at a program site. Thus, interviewers had no particular reason to be biased in their observations of the mothers interacting with their children.

[7] The magnitude of the impact can be quantified through a statistic known as the "effect size," which provides a standardized measure (in standard deviation units) of the strength of the impact. The effect size on the Emotional Support subscale in the New Chance sample was .09. In other words,the experimental group's score was higher than the control group's score by about 1/10 of a standard deviation. In contrast, among the subgroup of women in the Project Redirection five-year follow-up sample who were on welfare at baseline, the effect size on the total HOME scale was .73 (i.e., nearly 3/4 of a standard deviation difference) – an effect size about eight times as large as in New Chance.

[8] Existing scales were considered inappropriate for this study. They were generally found to be too long or unwieldy for administration in a survey, to use language that was considered too sophisticated for an educationally disadvantaged sample, or to include items that were oriented to the experiences of middle-class parents or two-parent families.

TABLE 7.1

IMPACTS OF NEW CHANCE ON HOME ENVIRONMENT AND PARENTING SCALES AT 18 MONTHS AFTER RANDOM ASSIGNMENT

Outcome	Experimentals	Controls	Difference	p (a)
Average score on HOME scale				
Cognitive Stimulation subscale	99.5	100.6	−1.1	0.118
Emotional Support subscale	100.6	99.3	1.3 *	0.056
Physical Environment subscale	100.8	100.5	0.3	0.665
Harsh Discipline subscale	100.6	100.0	0.6	0.387
Total HOME scale	100.5	100.3	0.2	0.732
Average score on parenting scales (b)				
Maternal Warmth/Responsiveness	23.5	23.4	0.1	0.867
Parenting Stress	27.6	28.2	−0.6	0.357
Maternal Control/Punitiveness	21.3	23.7	−2.4 ***	0.000
Mothers' rating of importance of spiritual/religious training for child (0 to 10 scale) (c, d)	7.6	7.2	0.4 *	0.056
Sample size	1,245	597		

SOURCES: MDRC calculations from New Chance Enrollment Form and survey data.

NOTES: A modified version of the short form of the Home Observation for Measurement of the Environment (HOME) Scale (first administered in the National Longitudinal Survey of Youth) was administered. Scores here were age−standardized to have a mean of 100 and a standard deviation of 15.

Calculations for this table used data for all 2,088 sample members for whom there were 18 months of follow−up survey data, including those with values of zero for outcomes and New Chance enrollees (i.e., experimentals) who did not participate in the program. The reported sample sizes may fall short of this number because of missing or unusable items from some sample members' questionnaires.

The averages are adjusted using linear analysis of covariance procedures controlling for up to 51 kinds of difference in characteristics before random assignment. Rounding may cause slight discrepancies in sums and differences.

(a) A two−tailed t−test was applied to each regression−adjusted difference between average experimental and control group outcomes. The column labeled "p" is the statistical significance level of the difference between experimental and control group outcomes: That is, p is the probability that average outcomes are different only because of random error. Statistical significance levels are indicated as *** = 1 percent; ** = 5 percent; * = 10 percent.

(b) The three self−report parenting scales were developed for this study.

(c) Ratings were on an 11−point scale, where 0 meant "not important at all" and 10 meant "extremely important."

(d) This question was asked of half the research sample, selected at random.

child's emotional needs. Despite the significant program impact on the Emotional Support subscale of the HOME, the experimental and control groups had comparable scores (just above 23) on the Maternal Warmth Scale. The great majority of mothers tended to describe themselves as warm and nurturing parents.[9]

The Parenting Stress Scale is an eight-item scale that measures the degree of stress or aggravation the mother perceives in relation to her interactions with the focal child. As shown in Table 7.1, the two groups had comparable scores on this scale: The mean for experimentals (27.6) was not significantly lower than the mean for controls (28.2). The mothers in this sample tended to report only a modest degree of stress.[10]

The Maternal Control/Punitiveness Scale is a six-item scale that is designed to tap the mother's authoritarian control over the child, versus a more democratic or permissive style of raising and disciplining the child. Table 7.1 shows that women in the experimental group had significantly lower average scores (21.3) than those in the control group (23.7), indicating a lower degree of self-reported authoritarian control.[11] Women in the experimental group were significantly less likely than controls to agree with an item that endorsed spanking as an effective way to gain the child's respect. Inasmuch as spanking and other forms of physical discipline were discouraged at most New Chance sites, the group difference on this scale may primarily reflect changes in stated views on spanking. However, it is noteworthy that the two groups did *not* differ significantly on the Harsh Discipline subscale of the HOME, a subscale that includes a question on actual self-reported spanking in the previous week, or with regard to interviewer observations of spanking during the interview session.[12] Thus, the women in the experimental group may have learned some childrearing lessons that they had not yet fully incorporated into their parenting practices – or their lower scores could simply indicate that they were more likely than controls to know the "right" answer to the questions. However, it is worth noting that in a recent analysis of observational data from a special study within the Teenage Parent Demonstration, mothers in the treatment (i.e., experimental) group were observed to be less authoritarian in their behavior toward their children than mothers in the control group (Brooks-Gunn and Berlin, 1993).

---

[9] An example of an item on this scale is: "Even when I'm in a bad mood, I show my child a lot of love." Scores on this scale could theoretically range from a low of 0 (maternal detachment or rejection) to a high of 30 (extreme maternal warmth). While actual scores did cover the full 0 to 30 range, only 9 percent of the sample received a score below the theoretical midpoint of 15, and the overall mean of 23.5 was well above that midpoint. The scale's internal consistency reliability was fairly low (.55).

[10] Examples of items on the Parenting Stress scale are: "I often feel angry with my child" and "My child seems to be much harder to care for than most." The internal consistency reliability of this scale for the New Chance sample was adequate (.70). The scores theoretically could range from a low of 0 (a total absence of stress, enjoyment of the parenting role) to a high of 80 (an extreme degree of parental stress and aggravation). Actual scores ranged from 0 to 71, and 80 percent of the mothers scored below the theoretical midpoint of 40, suggesting relatively low perceived parenting stress among these young mothers.

[11] Examples of items on this scale include the following: "When a parent asks a child to do something, the child should just do it without having to be told why" and "I think children must learn early not to cry." The internal consistency coefficient for this scale was fairly low (.60). Scores on the Maternal Control/Punitiveness Scale ran the full theoretical range from 0 (extremely low control and punitiveness) to 60 (extremely high control and punitiveness).

[12] The majority of women in both groups disagreed with two statements relating to spanking on the Maternal Control/Punitiveness Scale ("If a mother never spanks her child, the child won't learn respect" and "It is sometimes necessary to discipline a child with a good, hard spanking"). Nevertheless, most women (66.9 percent of those with a child under age 3, and 60.7 percent of those with a child aged 3 to 5) reported that they *had* spanked the focal child at least once – but on average three or four times – in the previous week. Interviewers observed the focal child being spanked during the course of the 90-minute interview session in 6.2 percent of the homes where the child was under age 6.

The final entry in Table 7.1 concerns the following question: "Aside from attending religious services, how important is it to you to provide spiritual or religious training for your child, using a 0 to 10 scale where zero means 'not important at all' and 10 means 'extremely important'?" Both groups rated spiritual instruction as moderately important, but the mean was significantly higher for the experimental group (7.6) than for the control group (7.2).[13] Program staff were not explicitly expected to address religious or spiritual issues, but it is possible that these issues emerged in the course of parenting classes or individual counseling sessions.

The New Chance program did not specifically try to influence any aspect of parenting in relation to the fathers (or father figures) of the children. However, in light of the finding that experimental group women were more likely to be living with a male partner at follow-up than were control group women (see Chapter 6), differences in paternal relations between the two groups were examined. The analyses revealed no significant group differences with regard to any of the paternal variables. The two groups were virtually identical with respect to the focal child's frequency of seeing his or her biological father (about 41 percent saw him at least once a week); receipt of monetary child support (about 40 percent received some in the previous year, and about 57 percent had ever received some); or receipt of in-kind assistance such as diapers, groceries, toys, etc. (about 56 percent in the previous year). The majority of children in both groups (about 86 percent) reportedly had either a father or father figure with whom they were in contact several times a week or more often (results not shown in tables).

In summary, the program's effects on parenting and the home environment, as measured in the 18-month survey, were very modest – although differences that were observed were all in a favorable direction. The most noteworthy finding was that participation in New Chance was associated with significantly better scores on the Emotional Support subscale of the HOME, but the magnitude of the effect was small.

B. Subgroup Impacts on HOME Scores

This section examines the impact of New Chance on the HOME scores for various subgroups of sample members. Because the aggregate group differences were significant only for the Emotional Support subscale, subgroup results are presented only for this subscale.[14]

---

[13] The two groups did not, however, differ with regard to the mothers' attendance at religious services. Nearly 40 percent of the sample said that they never attended religious services, and under 20 percent said that they attended at least once a week.

[14] Experimentals and controls had mean scores on the total HOME scale that were close to the overall mean of 100 for almost all subgroups. There was only one significant program effect: Total HOME scores

(continued...)

Table 7.2 indicates that the Emotional Support subscale scores were higher for the experimentals than for the controls for most of the subgroups examined, with differences reaching levels of statistical significance for 10 subgroups. Although the pattern of results is not totally consistent, it appears that the subgroups for which significant impacts were most likely to occur were ones with greater levels of disadvantage at baseline. For example, experimentals had significantly higher scores than controls among women who had more than one child at baseline, who were younger than 17 when they had their first child, who had been out of school for more than two years, or who were on welfare at baseline. In the five-year follow-up of Project Redirection, experimental-control group differences on HOME scores were similarly most pronounced among the subgroups of women who had originally been most disadvantaged (Polit, Quint, and Riccio, 1988).

The results for the subgroups defined on the basis of the focal child's gender were unanticipated. Table 7.2 indicates that the program impact was significant only when the focal child was a girl. The mean Emotional Support subscale score was 101.6 for experimentals whose focal child was a girl, compared to 98.2 for controls whose focal child was a girl. This impact of 3.4 points – which represents an effect size of .23 – constitutes the most sizeable subgroup impact on Emotional Support subscale scores. Moreover, these gender subgroups were the only ones where the between-subgroups impact difference was significant: The impact for boys was small and negative. The gender results are difficult to explain. They might mean that it is easier for programs to influence maternal behavior with girls, either because of differences in the behavior of young girls and boys or because of different cultural expectations about the ways boys and girls should be treated.

C. Site Effects on HOME Scores

The program's effects on the HOME Emotional Support subscale scores for individual sites are presented in Table 7.3, which shows that there was considerable inter-site variation.[15] Scores for both experimentals and controls were especially high in Chula Vista, Salem, and San Jose, and were especially low in Minneapolis and Portland.[16] The table also shows that experimentals received higher average scores than controls on the HOME Emotional Support subscale at most sites, and at several the difference was sizeable – though only at one site (Harlem) was the difference statistically significant. The between-sites impact difference was nonsignificant.

An effort was made to understand the factors that might have influenced site differences on the HOME Emotional Support subscale scores. In particular, information was examined to determine whether there might be any evidence of a relationship between a site's average performance on the

---

[14](...continued)
were higher in the experimental group (mean = 101.4) than in the control group (mean = 99.3) when the focal child was female ($p < .05$; not shown in tables).

[15] Site means for the total HOME score are not shown in the tables. The between-sites impact difference for total HOME scores was nonsignificant. However, in Jacksonville, the experimentals had significantly higher total HOME scores than controls (99.9 and 95.3, respectively).

[16] Since the Emotional Support subscale scores were based mainly on interviewer observation, one possibility for site differences on this subscale was differences in the harshness or leniency of interviewer ratings. However, similar site differences were also observed on other scales that were based on maternal self-report.

TABLE 7.2

IMPACTS OF NEW CHANCE ON THE EMOTIONAL SUPPORT SUBSCALE OF THE HOME SCALE AT 18 MONTHS AFTER RANDOM ASSIGNMENT, BY SUBGROUP

Characteristic and Subgroup at Random Assignment	Sample Size	Average Emotional Support Subscale Score Experimentals	Controls	Within-Subgroup Impact	p (a)	Between-Subgroups Impact Difference (b)	p (a)
Age (years)						--	0.761
16-17	355	100.0	98.5	1.5	0.372		
18-19	886	102.1	100.2	1.8 *	0.065		
20-22	599	98.8	98.2	0.7	0.582		
Ethnicity						--	0.319
Black, non-Hispanic	978	99.3	97.6	1.7 *	0.074		
Hispanic	415	102.5	103.1	-0.6	0.670		
White or other	446	101.7	99.5	2.3	0.113		
Living arrangement						0.7	0.617
Living with mother	642	100.9	99.2	1.7	0.159		
Not living with mother	1,178	100.5	99.5	0.9	0.289		
Number of children						1.6	0.288
1	1,206	101.6	100.8	0.8	0.363		
More than 1	636	98.7	96.4	2.3 **	0.047		
Age at first child's birth (years)						1.5	0.312
13-16	727	101.4	99.2	2.2 **	0.048		
17-19	1,115	100.1	99.3	0.8	0.383		
Child's gender (c)						3.9 ***	0.006
Girl	867	101.6	98.2	3.4 ***	0.001		
Boy	974	99.8	100.3	-0.5	0.605		
Child's age (years) (c)						-0.5	0.704
Less than 1	760	100.5	98.9	1.7	0.129		
1 or older	1,082	100.7	99.6	1.1	0.222		
Educational attainment						1.9	0.523
No high school diploma or GED	1,724	100.5	99.0	1.5 **	0.041		
Had a high school diploma or GED	114	102.3	102.8	-0.4	0.883		

(continued)

TABLE 7.2 (continued)

Characteristic and Subgroup at Random Assignment	Sample Size	Average Emotional Support Subscale Score		Within-Subgroup Impact	p (a)	Between-Subgroups Impact Difference (b)	p (a)
		Experimentals	Controls				
Highest grade completed						-0.1	0.959
10th or below	1,215	99.8	98.5	1.3	0.133		
11th or above	624	102.2	100.8	1.4	0.257		
Interval since last attended regular high school						1.7	0.246
More than 2 years	953	100.9	98.8	2.1 **	0.026		
2 years or less	834	100.4	99.9	0.5	0.640		
TABE reading test score (grade equivalent) (d)						--	0.911
Below 6th grade	374	99.1	98.4	0.7	0.648		
6th or 7th grade	428	99.9	97.8	2.1	0.151		
8th or 9th grade	518	101.5	100.4	1.1	0.418		
10th grade or above	516	101.4	99.7	1.6	0.213		
Ever employed						1.3	0.448
Yes	1,452	101.1	100.0	1.1	0.176		
No	390	98.9	96.6	2.4	0.118		
Prior-year earnings						-1.9	0.273
$0-$500	1,463	100.2	99.2	1.0	0.220		
$501 or more	369	102.5	99.6	2.9 *	0.067		
Any AFDC received in household						2.1	0.526
Yes	1,741	100.4	98.9	1.5 **	0.035		
No	98	104.8	105.3	-0.5	0.864		
Family received AFDC when sample member was growing up						--	0.931
Always	314	100.6	99.4	1.2	0.482		
Sometimes	853	100.0	98.8	1.3	0.215		
Never	662	101.4	99.6	1.8	0.126		

(continued)

TABLE 7.2 (continued)

Characteristic and Subgroup at Random Assignment	Sample Size	Average Emotional Support Subscale Score		Within-Subgroup Impact	p (a)	Between-Subgroups Impact Difference (b)	p (a)
		Experimentals	Controls				
CES-D (depression) Scale (e)						--	0.392
0-15 (not at risk)	864	100.8	99.3	1.5	0.146		
16-23 (at some risk)	477	101.4	98.9	2.5 *	0.063		
24-60 (at high risk)	497	99.5	99.5	-0.1	0.963		

SOURCES: MDRC calculations from New Chance Enrollment Form and survey data.

NOTES: A modified version of the short form of the Home Observation for Measurement of the Environment (HOME) Scale (first administered in the National Longitudinal Survey of Youth) was administered. Scores here were age-standardized to have a mean of 100 and a standard deviation of 15.

Calculations for this table used data for all 2,088 sample members for whom there were 18 months of follow-up survey data, including those with values of zero for outcomes and New Chance enrollees (i.e., experimentals) who did not participate in the program. The reported sample sizes may fall short of this number because of missing or unusable items from some sample members' questionnaires.

The averages are adjusted using a two-way analysis of covariance procedure controlling for up to 51 kinds of difference in characteristics, other than the characteristic used to define subgroups, before random assignment. The two categories used as factors were research status (i.e., membership in the experimental or control group) and, one at a time, the baseline characteristics indicated. Rounding may cause slight discrepancies in sums and differences.

(a) A two-tailed t-test was applied to each regression-adjusted within-subgroup impact and also, whenever there were two subgroups, to each difference between subgroup impacts. For each characteristic with more than two subgroups, an F-test was applied to the interaction between that characteristic and experimental or control status. The columns labeled "p" are the statistical significance levels of each within-subgroup impact and each between-subgroups difference in impacts: That is, p is the probability that sample estimates are different from zero or from each other only because of random error. Statistical significance levels are indicated as *** = 1 percent; ** = 5 percent; * = 10 percent.

(b) For each characteristic with only two subgroups, the between-subgroups impact difference is the impact for the first subgroup less the impact for the second subgroup. For characteristics with more than two subgroups, a between-subgroups impact difference cannot be calculated, as indicated by dashes in the table.

(c) For the 65 percent of sample members who had one child at random assignment, that child was the focus of all child-related questions on the 18-month survey, and is thus referred to in this report as the "focal child." The focal child for each sample member who had two or more children at random assignment was chosen at random from among those children.

(d) The test used to measure reading ability was the reading part of the Tests of Adult Basic Education (TABE). Most sites administered the Survey Form of the test, but some administered the full reading test.

(e) The Center for Epidemiological Studies Depression (CES-D) Scale is a widely used measure of depression; scores can range from zero to 60.

TABLE 7.3

IMPACTS OF NEW CHANCE ON EMOTIONAL SUPPORT SUBSCALE SCORES OF THE HOME SCALE AT 18 MONTHS AFTER RANDOM ASSIGNMENT, BY SITE

Site	Sample Size	Average Emotional Support Subscale Score: Experimentals	Average Emotional Support Subscale Score: Controls	Within-Site Impact	p (a)	Between-Sites Impact Difference p (a)
						0.843
Allentown	97	95.2	94.8	0.4	0.890	
Bronx	114	100.0	99.1	0.8	0.770	
Chicago Heights	62	98.3	95.5	2.8	0.486	
Chula Vista	113	107.6	106.5	1.1	0.710	
Denver	98	99.4	100.0	−0.6	0.855	
Detroit	147	102.5	104.4	−1.9	0.433	
Harlem	111	101.5	94.8	6.7 **	0.023	
Inglewood	121	105.6	103.8	1.8	0.495	
Jacksonville	125	100.8	98.4	2.4	0.366	
Lexington	118	98.7	97.6	1.1	0.694	
Minneapolis	115	91.9	91.8	0.1	0.963	
Philadelphia	119	97.6	93.1	4.5	0.101	
Pittsburgh	150	99.5	101.0	−1.5	0.537	
Portland	127	93.7	92.6	1.1	0.682	
Salem	119	107.4	103.7	3.7	0.179	
San Jose	106	109.0	108.0	1.0	0.725	
Sample size	1,842					

SOURCES: MDRC calculations from New Chance Enrollment Form and survey data.

NOTES: Calculations for this table used data for all 2,088 sample members for whom there were 18 months of follow-up survey data, including those with values of zero for outcomes and New Chance enrollees (i.e., experimentals) who did not participate in the program. The reported sample sizes may fall short of this number because of missing or unusable items from some sample members' questionnaires.

The averages are adjusted using a two-way analysis of covariance procedure controlling for up to 36 kinds of difference in characteristics, other than site, before random assignment. The two categories used as factors were research status (i.e., membership in the experimental or control group) and site. Rounding may cause slight discrepancies in sums and differences.

A modified version of the short form of the Home Observation for Measurement of the Environment (HOME) Scale (first administered in the National Longitudinal Survey of Youth) was administered. Scores here were age-standardized to have a mean of 100 and a standard deviation of 15.

(a) A two-tailed t-test was applied to each regression-adjusted within-site impact. An F-test was applied to the interaction between sites and experimental or control status. The columns showing p-values are the statistical significance levels of each within-site impact or between-sites impact difference: That is, p is the probability that sample estimates are different from zero or from each other only because of random error. Statistical significance levels are indicated as *** = 1 percent; ** = 5 percent; * = 10 percent.

subscale and site differences in the amount and quality of parenting education or child care services at the site level. These efforts (not shown in tables) proved not to be fruitful. Sizeable positive program impacts on the receipt of parenting education were observed across sites, but these effects did not uniformly translate into higher scores on the subscale. Moreover, site impacts on the subscale scores were not related to MDRC's field staff ratings of the program's conformity to parenting guidelines, nor to participants' ratings of how helpful the parenting classes were, nor to the presence of on-site child care.

D. HOME Scores and Program Participation

This section presents descriptive information regarding variations in scores on the HOME Emotional Support subscale in relation to levels of participation in New Chance. In these analyses, control group women were not included.

Scores were found to be positively correlated with the total amount of time spent in New Chance activities. The mean Emotional Support score for those with various levels of overall participation was as follows:

	Average Emotional Support Subscale Score
Zero hours in New Chance	98.7
Bottom third (1 to 127 hours)	99.1
Middle third (128 to 378 hours)	100.5
Top third (more than 378 hours)	102.9

Thus, the greater the level of involvement with New Chance, the better the scores. It is important to emphasize, as in previous chapters, that these descriptive findings cannot be interpreted as impacts (i.e., as resulting from the program). It is *possible* that greater exposure to New Chance helped participants foster a better home environment for their children. But it is equally plausible that young mothers who were already more attentive to their children's home environment were especially motivated to spend more time in New Chance.

The pattern of scores in relation to hours spent in parenting education classes was somewhat different, indicating a major distinction between having zero hours versus some hours of parenting:

	Average Emotional Support Subscale Score
Zero hours of parenting	98.4
Bottom third (1 to 9 hours)	101.3
Middle third (10 to 24 hours)	100.7
Top third (more than 24 hours)	101.7

Again, these findings are open to interpretation. However, since those who received 10 to 24 hours of parenting had lower scores than those who received 1 to 9 hours, the findings do not suggest that increasing the "dosage" of parenting classes in itself leads to improvements in mother-child interactions – unless a substantially higher dosage than was delivered is needed for a higher threshold of impacts.

III. Impacts on Child Care

Child care was an integral component of the New Chance model and was viewed as fulfilling two roles. First, it was considered essential to offer young mothers free, reliable, and convenient child care so that they would be able to participate regularly in the full schedule of New Chance activities. Second, in keeping with the two-generational focus of New Chance, child care was regarded as an important mechanism through which the child development needs of the participants' children could be addressed, given the evidence, cited at the beginning of this chapter, that high-quality child care programs can have positive and enduring effects on the development of disadvantaged children. Thus, programs were encouraged to offer on-site child care that was sensitive to the developmental milestones and emotional and cognitive needs of the children. At sites that were unable to offer on-site care, or in cases where the mothers themselves did not want to use on-site care, program staff were expected actively to assist the mothers in finding a no-cost child care arrangement that was compatible with full-time program participation.

On-site child care was available to New Chance participants in 12 of the 16 sites – although at two of these 12 sites, the child care facility provided only temporary or drop-in services, for emergency use only, and at a third the on-site care was used by only a very small number of New Chance participants because slots were not reserved for them. Sites without full-time on-site centers relied on linkages with a few child care centers that typically were located within a fairly short distance of the program.

As noted in Chapter 3, a special study of New Chance's child care facilities was undertaken. In this study, child care quality was assessed using both information provided in interviews with site staff (e.g., data on group size and child-to-staff ratios) and ratings of the quality of child care by specially trained observers. The findings indicate that the child care provided in most of the New Chance child care facilities was congruent with experts' guidelines for high-quality care, and that the facilities were generally of higher quality than the typical child care center that serves primarily low-income families.

This section examines patterns of post-baseline child care use for the focal child. Given the hypotheses that experimentals would be more heavily involved in education and training programs than controls, and given the program staff's mandate to assist participants with child care arrangements, it was anticipated that the two groups would differ with respect to both type of arrangements and the total amount of child care used in the follow-up period.

A. Aggregate Impacts on Child Care Use

Table 7.4 presents information on the sample members' use of regular child care arrangements for the focal child during the follow-up period. This table indicates that, even in the absence of New Chance, most of the mothers used some type of regular child care: 85.3 percent of controls' children were in a child care arrangement at some point during the follow-up period. This high use of child care is consistent with the fact that a high percentage of controls had participated in employment and

TABLE 7.4

IMPACTS OF NEW CHANCE ON CHILD CARE USE AT OR WITHIN 18 MONTHS AFTER RANDOM ASSIGNMENT, FOR THE FOCAL CHILD

Outcome	Experimentals	Controls	Difference	p (a)
Ever in any regular child care arrangement (b) (%)	95.3	85.3	10.1 ***	0.000
Ever in a child care arrangement before age 1 (%)	48.4	41.0	7.4 ***	0.001
Post-random assignment use of/ attendance in (%)				
School	7.3	7.5	-0.2	0.853
Head Start program	10.4	10.0	0.4	0.759
Day care center/preschool	63.4	33.4	30.0 ***	0.000
Family day care/unrelated babysitter	27.6	23.9	3.7 *	0.077
Care by a grandparent	43.0	43.4	-0.4	0.850
Care by another relative	22.2	25.4	-3.2	0.122
Care by husband/partner	19.0	19.8	-0.8	0.677
Average number of months since random assignment that child was in/used (c)				
Day care center/preschool	4.8	2.4	2.4 ***	0.000
Family day care/unrelated babysitter	1.5	1.2	0.3 *	0.055
Care by a grandparent	2.7	3.1	-0.5 *	0.053
Focal child in school/child care arrangement at follow-up (%)	50.7	53.7	-3.0	0.203
At follow-up, focal child in (%)				
Day care center/preschool	17.6	14.9	2.7	0.129
Care by a grandparent	16.3	18.8	-2.5	0.165
Sample size	1,298	634		

SOURCES: MDRC calculations from New Chance Enrollment Form and survey data.

NOTES: For the 65 percent of sample members who had one child at random assignment, that child was the focus of all child-related questions on the 18-month survey, and is thus referred to in this report as the "focal child." The focal child for each sample member who had two or more children at random assignment was chosen at random from among those children.

Calculations for this table used data for all 2,088 sample members for whom there were 18 months of follow-up survey data, including those with values of zero for outcomes and New Chance enrollees (i.e., experimentals) who did not participate in the program. The reported sample sizes may fall short of this number because of missing or unusable items from some sample members' questionnaires.

The averages or percentages are adjusted using linear analysis of covariance procedures controlling for up to 51 kinds of difference in characteristics before random assignment. Rounding may cause slight discrepancies in sums and differences.

(a) A two-tailed t-test was applied to each regression-adjusted difference between average experimental and control group outcomes. The column labeled "p" is the statistical significance level of the difference between experimental and control group outcomes: That is, p is the probability that average outcomes are different only because of random error. Statistical significance levels are indicated as *** = 1 percent; ** = 5 percent; * = 10 percent.

(b) Regular child care was defined as an ongoing arrangement used while the mother was in school, in training, or working.

(c) The number of months is the average number of months between random assignment and follow-up during which the child was in the specified arrangement. The average includes mothers who never used the specified arrangement and for whom the number of months would therefore have been zero.

training activities (or had held a job) during the follow-up period. However, even more of the mothers in the experimental group (95.3 percent) had made a child care arrangement for the focal child, and this group difference was statistically significant. Notably, the rate of non-maternal child care was extremely high in both groups in comparison to rates reported in other studies of disadvantaged young mothers.[17] Experimental group members' children were also significantly more likely than their control group counterparts to have entered a regular child care arrangement prior to their first birthday (48.4 percent versus 41.0 percent, respectively).

Among the control group members, the most frequently used type of non-maternal child care was care by a grandparent, reported by 43.4 percent of the controls. Essentially the same percentage of mothers in the experimental group (43.0 percent) had used a grandparent for child care. However, experimentals were nearly twice as likely as controls (63.4 percent versus 33.4 percent) to report having used a day care center or preschool program during the follow-up period. This finding is consistent with expectations, and undoubtedly reflects the use of on-site or program-referred child care centers by many mothers in the experimental group.[18] Mothers in the experimental group were also significantly more likely than mothers in the control group to have used a family day care or paid babysitting arrangement. The two groups did not differ significantly in terms of use of any of the other categories of care.[19] In sum, participation in the program had a substantial impact on the use of non-relative care for the focal child.[20]

Table 7.4 also shows the total length of time (in terms of actual duration of care) that the focal child was cared for in the three most commonly used arrangements. It should be noted that these numbers are the averages for all mothers, including those who did not use the specified arrangement and for whom the number of months would therefore have been zero; among those who actually used an arrangement, the averages were higher.[21] With respect to care by a grandparent, control group members' children spent significantly longer (about an extra two weeks, on average) than experimental group members' children. In contrast, children of controls spent only 2.4 months in a day care center, compared to 4.8 months by children of experimentals – a difference that was highly significant. Participation in the program was also associated with a significant increase in the amount of time children were cared for in a family day care home.

---

[17] For example, in the Teenage Parent Demonstration, which involved *mandatory* participation in various activities for those in the experimental group, the percentage of controls using child care during a 28-month follow-up period ranged from 55 percent in one site to 70 percent in another, and the percentage of experimentals using child care ranged from 67 to 80 percent (Maynard, Nicholson, and Rangarajan, 1993). Note that these figures are for the entire Teenage Parent Demonstration sample, not just for the subgroup that most resembled New Chance eligibles (i.e., school dropouts). However, the Teenage Parent Demonstration subgroup of dropouts had rates of participation in major activities about the same as those for the aggregate sample, so that child care use among this subgroup was likely to have been similar to that for the entire sample.

[18] Over half (52.9 percent) of the experimental group mothers said that they had used child care that was provided directly by the New Chance program. At several sites (Allentown, Detroit, Lexington, and Salem), 80 percent or more of the mothers had used on-site care (these findings are not shown in tables).

[19] Once again, these findings are in contrast to child care patterns for the Teenage Parent Demonstration sample. In that study, care by a relative was by far the most frequently used arrangement for both experimentals and controls. Center-based care was used by under 20 percent of the experimentals in all three sites – and by about 10 percent of the controls (Maynard, Nicholson, and Rangarajan, 1993). Thus, controls in the New Chance sample were more likely than even the experimentals in the Teenage Parent Demonstration sample to have used center-based care.

[20] Despite differences in types of care, mothers in both groups had mostly used arrangements that did not cost them any out-of-pocket money: Only 27.4 percent of the controls (26.2 percent of the experimentals) had ever paid directly for the child care they used.

The differences in child care had disappeared by the time the follow-up interviews were conducted. About half the children (53.7 percent of controls' children and 50.7 percent of experimentals' children) were in some type of school or child care arrangement at the time of the 18-month interview, a difference that was nonsignificant. Only about 6 percent of the women in the New Chance sample, with a comparable percentage in the two groups, reported that they had missed a day of work, school, or training in the previous month as a result of a problem with child care arrangements; among those who reported such a problem, however, the average number of days missed in the previous month was 4.2 (not shown in tables).

As indicated in Table 7.4, the two most prevalent types of arrangement at the time of the 18-month interview were care by a grandparent (18.8 percent of the controls and 16.3 percent of the experimentals) and care in a day care center or preschool (14.9 percent of the controls and 17.6 percent of the experimentals); the group differences for these two arrangements were not significant.[22] The rates of child care use at the time of the 18-month interview were similar to rates reported in the 1990 National Day Care Survey: About 54 percent of all children under age 5 had some type of regular non-maternal care (Hofferth et al., 1991). Nationally, however, more children (27 percent) were cared for in center-based care than was true for the New Chance sample.

Patterns of child care arrangements in the New Chance sample varied considerably over time, as shown in Figures 7.1 and 7.2. Figure 7.1 shows that the use of "market" child care (i.e., a day

---

[21] Among the focal children ever in a day care center, the mean number of months in such care during the follow-up period was 7.6 months for experimentals' children and 7.2 months for controls' children. Among those who had ever used family day care, the experimentals had their children in such care an average of 5.4 months, compared to 5.0 months for controls' children. Finally, among those cared for by grandparents, the average number of months in such an arrangement was 6.3 for the children of experimentals and 7.1 for the children of controls.

[22] At follow-up, just over 25 percent of the children in both groups were in an arrangement for at least 30 hours each week (not shown in tables). Other data (also not shown in tables) amplify the child care analysis. For example, when asked to rate on a 0 to 10 scale how satisfied they were with various aspects of their current child care arrangements – convenience, cost, reliability, and how much the child was learning – mothers generally expressed fairly high levels of satisfaction. Of the four dimensions, the mothers were most satisfied with the reliability of the arrangement (mean = 9.4) and least satisfied with how much their children were learning (mean = 8.4). Fourteen percent of the children in child care had a primary caretaker who did not have a high school diploma or GED; an additional 17 percent of the mothers did not know whether or not the provider had completed basic schooling. A minority of mothers in the sample (12.6 percent of the women in both groups) were paying out of their own pockets for a child care arrangement – for any child – at the time of the 18-month interview. Among the mothers with a child in child care, 73 percent had a cost-free arrangement. Among only those with any child care expenditure, the average was about $43 per week, ranging from $5 to $200.

FIGURE 7.1

USE OF "MARKET" CHILD CARE (A DAY CARE CENTER, PRESCHOOL, OR PAID BABYSITTER/FAMILY DAY CARE HOME), BY MONTH AFTER RANDOM ASSIGNMENT

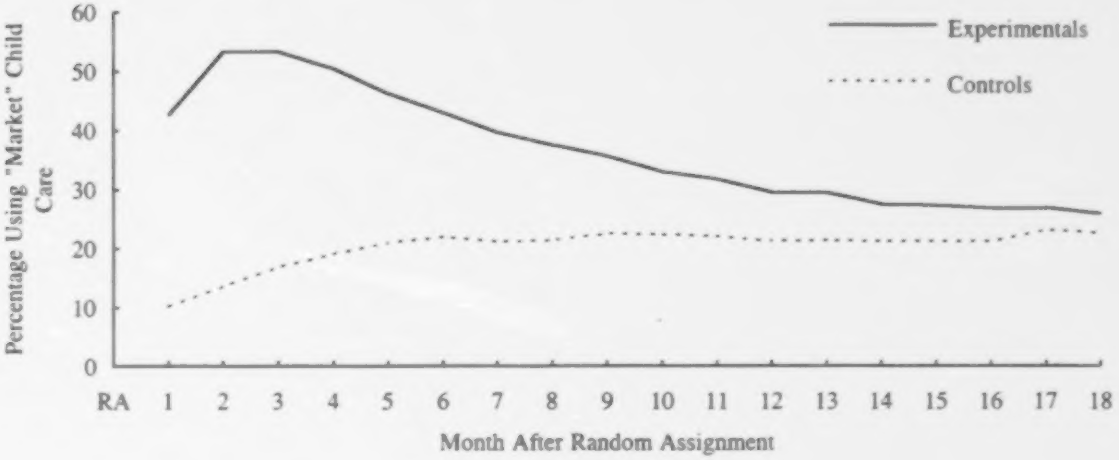
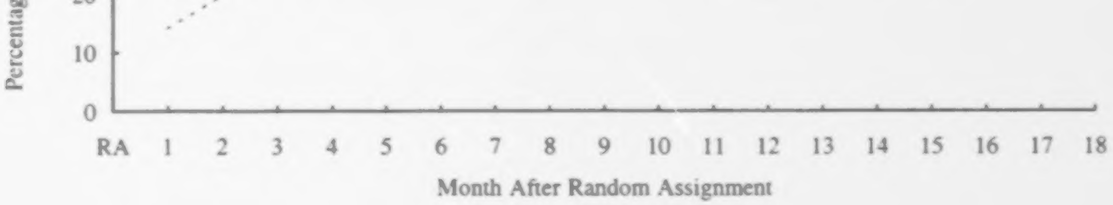


FIGURE 7.2

USE OF CHILD CARE PROVIDED BY A GRANDPARENT, ANOTHER RELATIVE, OR THE MOTHER'S HUSBAND OR PARTNER, BY MONTH AFTER RANDOM ASSIGNMENT



SOURCE: MDRC calculations from survey data.

care center, preschool, or paid babysitter/family day care home) was consistently higher in the experimental than in the control group. The experimental-control differences peaked by the second month after random assignment and then diminished over time. The experimentals' use of market care declined fairly steadily after the third month, while the controls' use of such care rose slightly but steadily. Nevertheless, the group difference was statistically significant in each month except month 18.

Figure 7.2 presents similar information with regard to the use of "relative" child care (i.e., care by a grandparent, other relative, or the mother's husband or partner) in the 18-month post-baseline period. Initially, experimentals were *more* likely than controls to use such care, with the difference attaining levels of significance in the first two months. By the sixth month, however, controls were significantly more likely to be using "relative" child care than were experimentals, and a significant difference persisted to month 18. These trends may reflect the fact that many controls, having been denied access to the New Chance program, waited several months before initiating an activity that required child care, while most experimentals needed child care soon after random assignment.

In conclusion, the child care experiences of the focal children of women in the two groups were quite different, particularly in the months immediately after random assignment. Focal children of experimentals spent more time in non-maternal child care arrangements than those of controls during the follow-up period, and were especially likely to spend time in "market" child care. Moreover, the children of experimentals were more likely to enter a regular child care arrangement as an infant. These different child care experiences might translate into different developmental patterns in the long run. However, the differences in child care use were of relatively short duration, and by the time of the 18-month interview, the differences had disappeared altogether – perhaps reflecting the experimentals' termination from New Chance. It remains to be seen whether spending several additional months, on average, in a child care center (and in child care centers of higher-than-average quality) could lead to different developmental outcomes. Given the controversy over the possibly negative effects of non-maternal child care for children under age 1 (Belsky and Eggebeen, 1991; Belsky, 1990; Fox and Fein, 1990), it will be especially interesting to examine program impacts on the developmental outcomes of the very young children of sample members when the 42-month follow-up data are available.

B. Subgroup Impacts on Child Care Use

For the sample as a whole, participation in New Chance was associated with higher levels of child care during the follow-up period, and especially with greater use of non-family arrangements. This section examines whether the aggregate effects on the use of a child care center or preschool were consistent across subgroups of the sample.

Program impacts on post-baseline (i.e., post-random assignment) use of a day care center or preschool were universally significant across subgroups, as shown in Table 7.5. The size of the experimental-control group difference was substantial in all cases, ranging from 22.3 percentage points (for women who were 16 or 17 at random assignment) to 41.5 percentage points (for women who were not receiving AFDC at baseline). Among the experimentals, differences from one subgroup to another were generally small: 59 percent or more of the experimentals in every subgroup had placed the focal child in a day care center or preschool during the follow-up period. Among the controls, in contrast, there was considerably more variability: Use of center care ranged from a low of 26.2 percent for

TABLE 7.5

IMPACTS OF NEW CHANCE ON USE OF A DAY CARE CENTER OR PRESCHOOL WITHIN 18 MONTHS AFTER RANDOM ASSIGNMENT, BY SUBGROUP

Characteristic and Subgroup at Random Assignment	Sample Size	Ever Used a Day Care Center or Preschool		Within-Subgroup Impact	p (a)	Between-Subgroups Impact Difference (b)	p (a)
		Experimentals (%)	Controls (%)				
Age (years)						--	0.240
16-17	384	64.4	42.2	22.3 ***	0.000		
18-19	921	65.0	34.1	30.9 ***	0.000		
20-22	626	60.4	27.5	33.0 ***	0.000		
Ethnicity						--	0.544
Black, non-Hispanic	1,018	64.5	35.7	28.8 ***	0.000		
Hispanic	436	61.1	32.8	28.2 ***	0.000		
White or other	476	63.3	28.9	34.4 ***	0.000		
Living arrangement						2.2	0.647
Living with mother	673	63.9	32.5	31.5 ***	0.000		
Not living with mother	1,238	63.2	33.9	29.3 ***	0.000		
Number of children						3.6	0.444
1	1,270	62.7	31.4	31.3 ***	0.000		
More than 1	662	64.7	37.0	27.7 ***	0.000		
Age at first child's birth (years)						-6.1	0.188
13-16	770	61.7	35.4	26.3 ***	0.000		
17-19	1,162	64.5	32.1	32.4 ***	0.000		
Child's gender (c)						8.7 *	0.052
Girl	917	63.0	28.6	34.4 ***	0.000		
Boy	1,015	63.6	38.0	25.7 ***	0.000		
Child's age (years) (c)						4.5	0.331
Less than 1	803	59.9	27.3	32.6 ***	0.000		
1 or older	1,129	65.8	37.7	28.1 ***	0.000		

(continued)

TABLE 7.5 (continued)

Characteristic and Subgroup at Random Assignment	Sample Size	Ever Used a Day Care Center or Preschool		Within-Subgroup Impact	p (a)	Between-Subgroups Impact Difference (b)	p (a)
		Experimentals (%)	Controls (%)				
Educational attainment						4.4	0.643
No high school diploma or GED	1,805	63.2	33.0	30.3 ***	0.000		
Had a high school diploma or GED	124	66.8	41.0	25.9 ***	0.005		
Highest grade completed						2.1	0.658
10th or below	1,267	62.0	31.2	30.8 ***	0.000		
11th or above	663	65.9	37.2	28.7 ***	0.000		
Interval since last attended regular high school						5.0	0.275
More than 2 years	1,005	63.1	30.7	32.4 ***	0.000		
2 years or less	871	64.1	36.8	27.4 ***	0.000		
TABE reading test score (grade equivalent) (d)						--	0.734
Below 6th grade	395	60.4	26.2	34.3 ***	0.000		
6th or 7th grade	445	66.6	37.0	29.6 ***	0.000		
8th or 9th grade	548	62.1	31.7	30.4 ***	0.000		
10th grade or above	539	64.3	37.4	26.9 ***	0.000		
Ever employed						1.6	0.771
Yes	1,529	64.3	34.0	30.3 ***	0.000		
No	403	59.8	31.1	28.7 ***	0.000		
Prior-year earnings						-0.7	0.903
\$0-\$500	1,541	63.5	33.9	29.6 ***	0.000		
\$501 or more	382	62.9	32.6	30.3 ***	0.000		
Any AFDC received in household						-12.0	0.250
Yes	1,827	62.9	33.5	29.5 ***	0.000		
No	103	72.0	30.5	41.5 ***	0.000		

(continued)

TABLE 7.5 (continued)

Characteristic and Subgroup at Random Assignment	Sample Size	Ever Used a Day Care Center or Preschool: Experimentals (%)	Ever Used a Day Care Center or Preschool: Controls (%)	Within-Subgroup Impact	p (a)	Between-Subgroups Impact Difference (b)	p (a)
Family received AFDC when sample member was growing up						--	0.701
Always	322	59.4	32.4	27.0 ***	0.000		
Sometimes	894	62.8	33.0	29.9 ***	0.000		
Never	701	66.1	33.5	32.6 ***	0.000		
CES-D (depression) Scale (e)						--	0.174
0-15 (not at risk)	908	61.0	35.2	25.8 ***	0.000		
16-23 (at some risk)	511	66.1	34.5	31.6 ***	0.000		
24-60 (at high risk)	510	64.7	28.9	35.8 ***	0.000		

SOURCES: MDRC calculations from New Chance Enrollment Form and survey data.

NOTES: Calculations for this table used data for all 2,088 sample members for whom there were 18 months of follow-up survey data, including those with values of zero for outcomes and New Chance enrollees (i.e., experimentals) who did not participate in the program. The reported sample sizes may fall short of this number because of missing or unusable items from some sample members' questionnaires.

The averages or percentages are adjusted using a two-way analysis of covariance procedure controlling for up to 51 kinds of difference in characteristics, other than the characteristic used to define subgroups, before random assignment. The two categories used as factors were research status (i.e., membership in the experimental or control group) and, one at a time, the baseline characteristics indicated. Rounding may cause slight discrepancies in sums and differences.

(a) A two-tailed t-test was applied to each regression-adjusted within-subgroup impact and also, whenever there were two subgroups, to each difference between subgroup impacts. For each characteristic with more than two subgroups, an F-test was applied to the interaction between that characteristic and experimental or control status. The columns labeled "p" are the statistical significance levels of each within-subgroup impact and each between-subgroups difference in impacts: That is, p is the probability that sample estimates are different from zero or from each other only because of random error. Statistical significance levels are indicated as *** = 1 percent; ** = 5 percent; * = 10 percent.

(b) For each characteristic with only two subgroups, the between-subgroups impact difference is the impact for the first subgroup less the impact for the second subgroup. For characteristics with more than two subgroups, a between-subgroups impact difference cannot be calculated, as indicated by dashes in the table.

(c) For the 65 percent of sample members who had one child at random assignment, that child was the focus of all child-related questions on the 18-month survey, and is thus referred to in this report as the "focal child." The focal child for each sample member who had two or more children at random assignment was chosen at random from among those children.

(d) The test used to measure reading ability was the reading part of the Tests of Adult Basic Education (TABE). Most sites administered the Survey Form of the test, but some administered the full reading test.

(e) The Center for Epidemiological Studies Depression (CES-D) Scale is a widely used measure of depression; scores can range from zero to 60.

women who were reading below the 6th grade level at baseline to 42.2 percent for those who were 16 or 17 at baseline.

Generally, impacts tended to be somewhat larger among the more disadvantaged subgroups (e.g., those with low reading scores or high depression scores at baseline), but the between-subgroups impact difference was significant for only one set of subgroups: those defined by the gender of the focal child. In this sample, control group women were somewhat more likely to use a child care center or preschool for boys (38.0 percent) than for girls (28.6 percent), whereas the gender of the child had little effect among the experimentals.

C. Site Impacts on Child Care Use

Table 7.6 shows that a higher percentage of experimentals than controls had used a day care center or preschool after random assignment at every site, and significantly so at most of them (13 of the 16 sites). Differences in the magnitude of site impacts were substantial, and the between-sites impact difference was highly significant. This reflects the fact that the experimental-control group difference was very small at a few sites (e.g., a 1.6 percentage point difference in San Jose) and quite large at others (e.g., a 63.5 percentage point difference in Salem).

There does not appear to be a consistent relationship between the magnitude of the impact and the presence of an on-site day care center. For example, the lowest impact was in San Jose, which has an on-site day care center – although one rarely used by New Chance participants because slots were not reserved for them. In contrast, at four of the six sites *without* an on-site facility (Inglewood, Jacksonville, Minneapolis, and Philadelphia), the experimental-control group difference approached or exceeded 25 percentage points. However, the most sizeable impacts *were* observed at two sites with on-site child care: Detroit (51.1 percentage points) and Salem (63.5 percentage points). Thus, on-site child care appears to have contributed to the observed impacts, but having on-site care does not completely account for the higher use of center care among the experimental group.

IV. Impacts on Child Health

The children in this sample, all of whom came from disadvantaged families, were expected to be at higher-than-average risk of health problems, given the evidence linking poverty to a broad range of poor health outcomes in children (Dawson, 1991; Hughes et al., 1989; Mott and Quinlan, 1991). According to program guidelines, New Chance programs were required to provide free, on-site health care to participants and their children or to create linkages with specific hospitals and clinics to which they could be referred. On-site health care was available at three sites, and others forged linkages with health care providers. In addition, child health issues were covered in both the health education and parenting components of the program. For example, classes sometimes covered such topics as infant nutrition, hygiene, and childhood immunizations.

This section examines whether there were any program impacts on indicators of child health or health care for the focal child at 18 months after random assignment. As was true for maternal health measures (Chapter 6), the child health measures were limited and were based entirely on the mothers' reports rather than on objective physiological information or data from medical records.

TABLE 7.6

IMPACTS OF NEW CHANCE ON USE OF A DAY CARE CENTER OR PRESCHOOL WITHIN 18 MONTHS AFTER RANDOM ASSIGNMENT, BY SITE

Site	Sample Size	Ever Used a Day Care Center or Preschool		Within-Site Impact	p (a)	Between-Sites Impact Difference p (a)
		Experimentals (%)	Controls (%)			
						*** 0.000
Allentown	102	96.7	55.4	41.3 ***	0.000	
Bronx	118	38.1	33.6	4.6	0.619	
Chicago Heights	59	25.1	19.8	5.3	0.688	
Chula Vista	118	64.5	34.9	29.5 ***	0.001	
Denver	100	81.6	42.7	38.9 ***	0.000	
Detroit	155	69.7	18.6	51.1 ***	0.000	
Harlem	115	50.6	24.3	26.3 ***	0.005	
Inglewood	124	57.7	33.1	24.6 ***	0.005	
Jacksonville	133	77.3	38.4	38.9 ***	0.000	
Lexington	121	80.0	51.9	28.1 ***	0.002	
Minneapolis	117	69.9	41.5	28.5 ***	0.002	
Philadelphia	130	58.4	32.8	25.6 ***	0.003	
Pittsburgh	157	59.0	27.9	31.1 ***	0.000	
Portland	131	51.9	27.4	24.5 ***	0.004	
Salem	126	80.3	16.8	63.5 ***	0.000	
San Jose	126	41.1	39.5	1.6	0.855	
Sample size	1,932					

SOURCES: MDRC calculations from New Chance Enrollment Form and survey data.

NOTES: Calculations for this table used data for all 2,088 sample members for whom there were 18 months of follow-up survey data, including those with values of zero for outcomes and New Chance enrollees (i.e., experimentals) who did not participate in the program. The reported sample sizes may fall short of this number because of missing or unusable items from some sample members' questionnaires.

The percentages are adjusted using a two-way analysis of covariance procedure controlling for up to 36 kinds of difference in characteristics, other than site, before random assignment. The two categories used as factors were research status (i.e., membership in the experimental or control group) and site. Rounding may cause slight discrepancies in sums and differences.

(a) A two-tailed t-test was applied to each regression-adjusted within-site impact. An F-test was applied to the interaction between sites and experimental or control status. The columns showing p-values are the statistical significance levels of each within-site impact or between-sites impact difference: That is, p is the probability that sample estimates are different from zero or from each other only because of random error. Statistical significance levels are indicated as *** = 1 percent; ** = 5 percent; * = 10 percent.

Mothers were asked to characterize the focal child's health as "excellent," "very good," "good," "fair," or "poor." As shown in Table 7.7, nearly 80 percent of the mothers in both groups described the child's health as either "excellent" or "very good." About 50 percent of the women said that their child's health was "excellent," and under 1 percent of the sample said that the child's health was "poor" (not shown in the table).[23]

The mothers reported that, over the 18-month follow-up period, there were just under three days, on average, during which the child had spent more than half the day in bed because of illness or injury. The average number of illness days for the two groups was nearly identical. The number of such days ranged from 0 to 150 and was severely skewed, with about 50 percent of the mothers reporting none and about 6 percent reporting more than 10 (not shown in tables).

Despite the fairly positive picture of health suggested by the health ratings and despite the young age of the focal child, about one out of every eight children of both experimentals and controls had been hospitalized (i.e., had been a patient in a hospital overnight or longer) at least once during the year and a half since baseline. Among the children who had ever been hospitalized, more than one out of four (27.9 percent) had been hospitalized on two or more different occasions during the follow-up period (not shown in tables).

Nearly one-fourth of the children (22.4 percent of experimentals' children and 23.5 percent of controls' children) had had an injury, poisoning, or accident since random assignment that was severe enough to require medical attention. The group difference was not statistically significant. It should be noted, however, that the rate for both groups appears to have been high relative to children nationally. In the 1988 NLSY survey, for example, 11.1 percent of the poor children under age 5 were reported to have had an accident or injury that required medical attention in the previous 12 months (Mott and Quinlan, 1991). The NLSY rate of under 1 percent per child-month (11.1 percent divided by 12 months equals .93 percent) having such an injury is 28.5 percent lower than the New Chance rate of 1.3 percent per child-month (22.8 percent for the New Chance sample divided by 18 months equals 1.3 percent). This is consistent with several studies that have found that the children of young mothers are at greater risk of injuries and fatalities than those with older mothers (Emerick, Foster, and Campbell, 1986; Strobino, Alexander, and Kim, 1988; Winpisinger et al., 1991).

Mothers were also asked several questions about the health care of the focal child. First, they were asked whether there is "a particular clinic, health center, doctor's office, or other place" that they usually went to when the child was sick or if they needed advice about the child's health. As shown in Table 7.7, 95.6 percent of the controls, but 97.2 percent of the experimentals, said that they had such a health care provider, a difference that was statistically significant. This suggests that the program was helpful in linking participants and their children to a health care provider, but the magnitude of the increment was small because of high rates in the control group.

---

[23] To put this in context, information from the National Health Interview Survey indicates that, in 1986, 41 percent of poor children in the United States under the age of 5 (i.e., children in families with incomes under $10,000) were described as being in excellent health; 64 percent of same-aged children in the most affluent category (with family incomes over $35,000) were in excellent health (Hughes et al., 1989).

TABLE 7.7

IMPACTS OF NEW CHANCE ON CHILDREN'S HEALTH AND HEALTH CARE AT OR WITHIN 18 MONTHS AFTER RANDOM ASSIGNMENT

Outcome for Focal Child (a)	Experimentals	Controls	Difference	p (b)
Mother rated focal child's health as "excellent" or "very good" (%)	78.5	79.1	-0.6	0.767
Average number of days child stayed in bed more than half a day due to illness or injury since random assignment	2.8	2.7	0.1	0.757
Child hospitalized at least once since random assignment (%)	13.6	13.6	0.0	0.998
Child had an injury, poisoning, or accident that required medical attention since random assignment (%)	22.4	23.5	-1.1	0.594
Mother had a particular doctor or clinic to go to when child was sick (%)	97.2	95.6	1.6 *	0.059
Mother had a health care provider who knew child and could give medical advice over the phone (%)	80.0	79.1	0.9	0.630
Child had either Medicaid/Medi-Cal or private health insurance plan (%)	91.9	90.5	1.4	0.282
Sample size	1,298	634		

SOURCES: MDRC calculations from New Chance Enrollment Form and survey data.

NOTES: Calculations for this table used data for all 2,088 sample members for whom there were 18 months of follow-up survey data, including those with values of zero for outcomes and New Chance enrollees (i.e., experimentals) who did not participate in the program.

The averages or percentages are adjusted using linear analysis of covariance procedures controlling for up to 51 kinds of difference in characteristics before random assignment. Rounding may cause slight discrepancies in sums and differences.

(a) For the 65 percent of sample members who had one child at random assignment, that child was the focus of all child-related questions on the 18-month survey, and is thus referred to in this report as the "focal child." The focal child for each sample member who had two or more children at random assignment was chosen at random from among those children.

(b) A two-tailed t-test was applied to each regression-adjusted difference between average experimental and control group outcomes. The column labeled "p" is the statistical significance level of the difference between experimental and control group outcomes: That is, p is the probability that average outcomes are different only because of random error. Statistical significance levels are indicated as *** = 1 percent; ** = 5 percent; * = 10 percent.

Although mothers in the experimental group were more likely to have had a particular provider for their child, they were as likely as controls to say that they knew the provider sufficiently well that they could get medical advice for the child over the telephone. About 80 percent of the mothers in both groups said that they had such a relationship with a provider.

Finally, the survey asked mothers about health insurance (both Medicaid and private) that covered the child's medical expenses. The overwhelming majority of children in both groups (91.9 percent of experimentals' children and 90.5 percent of controls' children) had health care insurance at the time of the 18-month interview. The group difference was not significant.[24]

Thus, on the indicators included in the 18-month survey, the health and health care of the children in the two groups were largely similar. An exception is that experimental group mothers were significantly more likely to have had a regular health care provider than mothers in the control group, but the absolute difference was small. Overall, it appears that the children in the sample may have had more health problems than the average child – a fact that is not surprising given the level of disadvantage of these families.

---

[24] About 88 percent of the sample was covered by Medicaid, and 10.6 percent had private insurance, with 5.7 percent having had both types of coverage (not shown in tables). In contrast, among poor children under age 5 in the 1988 NLSY survey, 51 percent were covered by Medicaid and 29 percent had private health care insurance (Mott and Quinlan, 1991). Thus, in the New Chance sample, health care coverage appears to have been broader than is true nationally for young children, reflecting the fact that most of the New Chance sample members were AFDC recipients who were eligible for Medicaid.

CHAPTER 8

IMPACTS ON EMPLOYMENT, EARNINGS, WELFARE RECEIPT, AND FAMILY INCOME

I. Introduction

A primary objective of New Chance is to improve the labor market prospects of a group of women who would otherwise be at high risk of long-term welfare receipt. The program attempts to do this by focusing on up-front education and training rather than immediate job placement. Specifically, it provides intensive education and training services to help participants increase their skills and success in the labor market, stimulates the attainment of education credentials, and offers other employment-related activities such as employability development classes and work internships. Overall, program planners targeted a group of young women who they anticipated would have very limited employment experience and sought to provide them with enough human capital to secure jobs that would set them on the path to long-term self-sufficiency.

As was pointed out in Chapter 2, the program's intended labor market gains were not expected to appear immediately after young women entered New Chance. Initially, it was recognized, participants' education and training activities might prevent them from seeking employment. The resulting short-term reductions in earnings – if they occurred – would be an "opportunity cost" of attending the program.[1]

A. The Scope of This Chapter

This chapter describes the early (18-month) employment experiences of the New Chance sample. It covers both the in-program period, during which any earnings losses attributable to participating in the program would have become apparent, and early post-program experiences, which would not have been expected to show the long-term employment effects the program set out to achieve. The chapter also compares the extent of welfare receipt by experimentals and controls as well as the two groups' sources of income. It then discusses the program's impact on "skill-building activity," a measure that encompasses employment, education and training activities, and unpaid work experience. It ends by briefly comparing some of these findings with the results from two other demonstrations that included disadvantaged young mothers.

B. A Preview of the Findings

During the 18 months of follow-up, a substantial share of both New Chance experimentals and controls found jobs, although many left them quite rapidly. In the first six to 12 months, controls were more likely to have been working, and they earned more than experimentals. This difference narrowed throughout the follow-up period, however, as New Chance participants left the program and entered the labor market. AFDC receipt rates were comparably high for experimentals and controls during most of the 18 months, with more than four-fifths of both groups still being on welfare at the end of the follow-up period. Overall, there was also no impact on the young women's receipt of income from sources other than welfare. Finally, experimentals spent more time than controls in skill-building activities such as employment and education, and training activities, but the difference diminished over time.

---

[1] "Opportunity cost" is a term economists use to describe an indirect kind of "cost" associated with a course of action such as participating in a program: the loss of the opportunity to do other things instead. A potential opportunity cost for New Chance participants was forgone earnings they might have gotten from working during some or all of the hours in which they were attending the program. Another was the cost in time not spent taking care of their children, although the availability of good-quality care in New Chance lessens the pertinence of this issue.

II. Impacts on Employment Rates

As was shown in Table 2.1, 63.3 percent of sample members had not been employed in the 12 months prior to applying to New Chance, and only 20.1 percent of the sample had earned more than $500 during that period.

Table 8.1 shows the impact of New Chance on rates of employment. Quarterly employment rates among controls increased slowly throughout the follow-up period. In the first three months after random assignment (quarter 1), only 12.7 percent of the controls were employed, while in the last three months before the follow-up interview (quarter 6), 26.3 percent were employed.[2] The employment rate over the full 18 months was much higher (44.9 percent for controls), suggesting that many sample members left or lost jobs at some point in the follow-up period. This finding is not unique to New Chance. Another study (Pavetti, 1992) found similarly high job turnover in a nationally representative sample of young mothers on welfare, as did a study of several programs that targeted a broader range of welfare recipients (Friedlander and Burtless, forthcoming). Job turnover rates have been found to be higher for welfare recipients who do not have education credentials (Pavetti, 1992), as was the case for the majority of women in the New Chance sample.

During the early part of the follow-up period, the effect of New Chance on employment rates was mostly negative. In the first two quarters after random assignment, employment rates among experimentals were 4.7 and 4.8 percentage points lower than those among controls, differences that were statistically significant. Especially during those first six months, the program in the main encouraged its participants to focus on attaining their GEDs. After the first six months, employment rate differences became smaller and lost their statistical significance, disappearing completely by the end of the follow-up period.

The next panel of Table 8.1 further explores sample members' post-baseline labor market experiences with a measure of how soon they started their first job after random assignment. Each of the three rows in this panel shows the percentage of experimentals and controls who were first

---

[2] As noted in Chapter 1, the JOBSTART Demonstration was targeted at high school dropouts with low levels of reading skills. In a subgroup of women who were living with their own children at baseline, the controls reported an employment rate of 15.1 percent in the first quarter after random assignment, and an employment rate of 28.7 percent in the sixth quarter. These percentages are only slightly higher than those for the New Chance controls (Cave et al., 1993).

TABLE 8.1

IMPACTS OF NEW CHANCE ON EMPLOYMENT RATES WITHIN 18 MONTHS AFTER RANDOM ASSIGNMENT

Outcome and Follow-Up Period	Experimentals	Controls	Difference	p (a)
Ever employed (%)				
Quarter 1 (b)	8.0	12.7	-4.7 ***	0.001
Quarter 2	14.4	19.2	-4.8 ***	0.004
Quarter 3	17.9	20.3	-2.4	0.185
Quarter 4	18.5	21.2	-2.7	0.126
Quarter 5	22.3	23.8	-1.5	0.449
Quarter 6	26.8	26.3	0.4	0.826
Quarters 1-4	27.7	33.2	-5.5 ***	0.007
Quarters 1-6	42.6	44.9	-2.2	0.311
First employed (%)				
Quarters 1-2	15.2	21.2	-6.0 ***	0.001
Quarters 3-4	12.5	12.0	0.5	0.767
Quarters 5-6	14.9	11.6	3.3 **	0.043
Average number of jobs				
Quarters 1-6	0.7	0.8	-0.1 **	0.02
Sample size	1,408	680		

SOURCES: MDRC calculations from New Chance Enrollment Form and survey data.

NOTES: Calculations for this table used data for all 2,088 sample members for whom there were 18 months of follow-up survey data, including those with values of zero for outcomes and New Chance enrollees (i.e., experimentals) who did not participate in the program.

The averages or percentages are adjusted using linear analysis of covariance procedures controlling for up to 51 kinds of difference in characteristics before random assignment. Rounding may cause slight discrepancies in sums and differences.

(a) A two-tailed t-test was applied to each regression-adjusted difference between average experimental and control group outcomes. The column labeled "p" is the statistical significance level of the difference between experimental and control group outcomes: That is, p is the probability that average outcomes are different only because of random error. Statistical significance levels are indicated as *** = 1 percent; ** = 5 percent; * = 10 percent.

(b) Quarter 1 refers to the three calendar months beginning with the month in which the sample member was randomly assigned to the experimental or control group. Thus, e.g., for a young woman who was randomly assigned on May 16, 1990, quarter 1 means the period from May 1 through July 31, 1990.

employed in three six-month periods following random assignment. While the differences between experimentals and controls were fairly small, experimentals were more likely than controls to have begun working later in the follow-up period. On average, experimentals *who worked* during the follow-up period (not shown in the table) entered their first job 8.4 months after random assignment, whereas controls who worked entered theirs 7.0 months into the follow-up period.[3]

Table 8.1 also shows the average number of jobs reported by sample members. This measure includes zeros for those who did not work during the follow-up period. The program appears to have had a significant negative impact on the number of jobs obtained, independent of the overall employment rate (which did not differ significantly between experimentals and controls). On average, experimentals *who worked* (not shown in the table) had 1.6 jobs, whereas controls who worked had 1.8 jobs. This difference could signal greater job stability among experimentals, but it could also be the result of controls being in the job market longer.

III. Impacts on Hours and Weeks Worked

Table 8.2 shows weeks and hours worked during the follow-up period. Again, these numbers include zeros for sample members who reported that they had not been employed; therefore, the results reflect the low employment rates in the sample as a whole. Overall, control group members worked an average of 10.8 weeks during the 78-week (18-month) follow-up period. This amounts to 24.1 weeks per worker (not shown in the table). The number of hours worked were closely related to the number of weeks worked, and amounted to an average of 340.6 hours for the control group during the follow-up period. Dividing the average number of hours worked by the average number of weeks worked produces an average of 31.4 hours worked per average week worked (not shown in the table). During the 18-month follow-up interviews, 21.7 percent of the controls reported that they had worked in the previous month; of that number, 45.5 percent reported that they had worked 25 hours or fewer per week during that month, and 34.5 percent reported having worked more than 35 hours per week (not shown in the table).

The program's impacts on weeks and hours worked closely followed the impacts on employment rates discussed above, with controls having worked more. However, impacts on hours worked were more persistent than impacts on employment rates. Differences in hours worked were statistically significant throughout the first year of the follow-up period. This is because experimentals, when they were working, worked (on average) fewer weeks and hours than controls. While the average control group member *who worked* during the follow-up period worked 24.1 weeks, the average experimental who worked did so for only 21.3 weeks. Hours worked per week worked were also lower for

---

[3] This comparison of the experiences of experimentals who worked and controls who worked did not involve the full sample. As a result, it was subject to selection bias: Experimentals who worked may have been different from controls who worked in ways that were not controlled for by the experimental design of the study. Consequently, the results from this comparison and from others that were not based on the full sample cannot be interpreted as program impacts. The program impact on time until first post-baseline employment was re-estimated for the full sample (including those who had never worked during the follow-up period) using a Tobit estimator. Such a procedure adjusts the estimate for truncation at the end of the follow-up period. This procedure also found a statistically significant impact of 1.4 months.

TABLE 8.2

IMPACTS OF NEW CHANCE ON HOURS AND WEEKS WORKED WITHIN 18 MONTHS AFTER RANDOM ASSIGNMENT

Outcome and Follow-Up Period	Experimentals	Controls	Difference	p (a)
Average number of weeks employed				
Quarter 1 (b)	0.7	1.1	-0.4 ***	0.002
Quarter 2	1.2	1.7	-0.5 ***	0.002
Quarter 3	1.6	1.8	-0.2	0.180
Quarter 4	1.7	1.9	-0.2	0.209
Quarter 5	1.9	2.2	-0.2	0.229
Quarter 6	2.1	2.3	-0.2	0.384
Quarters 1-4	5.0	6.4	-1.4 ***	0.010
Quarters 1-6	9.1	10.8	-1.8 **	0.023
Average total hours worked				
Quarter 1	18.4	30.7	-12.3 ***	0.002
Quarter 2	34.7	51.3	-16.6 ***	0.002
Quarter 3	45.2	59.6	-14.4 **	0.018
Quarter 4	47.8	60.6	-12.8 **	0.034
Quarter 5	59.1	66.5	-7.4	0.262
Quarter 6	64.3	72.0	-7.6	0.252
Quarters 1-4	146.1	202.1	-56.0 ***	0.001
Quarters 1-6	269.6	340.6	-71.0 ***	0.005
Average hours worked per week during Quarters 1-6	3.5	4.4	-0.9 ***	0.005
Sample size	1,408	680		

SOURCES: MDRC calculations from New Chance Enrollment Form and survey data.

NOTES: Calculations for this table used data for all 2,088 sample members for whom there were 18 months of follow-up survey data, including those with values of zero for outcomes and New Chance enrollees (i.e., experimentals) who did not participate in the program.

The averages or percentages are adjusted using linear analysis of covariance procedures controlling for up to 51 kinds of difference in characteristics before random assignment. Rounding may cause slight discrepancies in sums and differences.

(a) A two-tailed t-test was applied to each regression-adjusted difference between average experimental and control group outcomes. The column labeled "p" is the statistical significance level of the difference between experimental and control group outcomes: That is, p is the probability that average outcomes are different only because of random error. Statistical significance levels are indicated as *** = 1 percent; ** = 5 percent; * = 10 percent.

(b) Quarter 1 refers to the three calendar months beginning with the month in which the sample member was randomly assigned to the experimental or control group. Thus, e.g., for a young woman who was randomly assigned on May 16, 1990, quarter 1 means the period from May 1 through July 31, 1990.

experimentals who worked: 29.7 hours on average versus 31.4 hours for controls who worked.[4] Again, these findings are consistent with the hypothesis that New Chance reduced participants' initial involvement in the labor market. Throughout the early part of the follow-up period, experimentals were significantly more likely to have combined work and education than were controls, and to have worked fewer hours and weeks.

IV. Impacts on Earnings

A. Aggregate Impacts on Earnings

Table 8.3 presents respondents' average quarterly earnings during the follow-up period. The earnings trend closely followed the trend in hours worked.[5] Both experimentals and controls earned more at the end of the follow-up period than they did initially, but the absolute levels remained modest. The program's impacts on earnings reflected the reduction in hours worked caused by the program. In the 18 months following random assignment, experimentals earned a total of $342 less than controls, a difference that was statistically significant. (Again, these averages include zeros for those who had no earnings.) In each of the six follow-up quarters, experimentals earned less than controls, but only in the first year (quarters 1-4) were these quarterly differences statistically significant.

It is too early to detect a possible payoff from the increased investment in education and training made by New Chance participants relative to the investment made by controls. The 42-month follow-up survey will be more likely to uncover such a payoff if it occurs. However, because employment and earnings in the New Chance control group are fairly low, the opportunity cost of participation is modest also. If the program were to achieve positive earnings impacts over the 24 months remaining until the next follow-up interview, the program's payoff to participants may ultimately become sufficient to offset this cost.

B. Impacts on Earnings for Selected Subgroups

Table 8.4 presents the program's impacts on average total 18-month earnings for selected subgroups, defined by baseline characteristics (i.e., characteristics at random assignment). Many of the differences across the subgroups were not statistically significant, but generally they did follow a consistent pattern. Young women who had more work experience faced higher opportunity costs – i.e., a larger earnings loss vis-à-vis controls – than women with no recent work experience. Similarly, women facing barriers to employment (such as having very young children or low reading

---

[4] Because these comparisons did not involve the full sample, they should not be regarded as impacts. Tests of statistical significance were not conducted.

[5] Some experimentals got paid, subsidized work internships. It was not possible to exclude the earnings from these internships from the estimates presented here because no data on subsidized work were available for controls. For experimentals, the subsidized component of their earnings was estimated to have been $103, or about 7.4 percent of their total earnings in the follow-up period. This estimate included all monthly earnings reported in months when New Chance participants were in a paid work internship. Therefore, the estimate is an upper bound.

TABLE 8.3

IMPACTS OF NEW CHANCE ON EARNINGS WITHIN 18 MONTHS AFTER RANDOM ASSIGNMENT

Outcome and Follow-Up Period	Experimentals ($)	Controls ($)	Difference	p (a)
Average earnings				
Quarter 1 (b)	92	152	-60 ***	0.006
Quarter 2	174	239	-65 **	0.023
Quarter 3	226	294	-68 **	0.039
Quarter 4	237	302	-65 **	0.044
Quarter 5	307	340	-33	0.387
Quarter 6	330	381	-51	0.187
Quarters 1-4	729	987	-258 ***	0.007
Quarters 1-6	1,366	1,708	-342 **	0.019
Sample size	1,408	680		

SOURCES: MDRC calculations from New Chance Enrollment Form and survey data.

NOTES: Calculations for this table used data for all 2,088 sample members for whom there were 18 months of follow-up survey data, including those with values of zero for outcomes and New Chance enrollees (i.e., experimentals) who did not participate in the program.

The averages or percentages are adjusted using linear analysis of covariance procedures controlling for up to 51 kinds of difference in characteristics before random assignment. Rounding may cause slight discrepancies in sums and differences.

(a) A two-tailed t-test was applied to each regression-adjusted difference between average experimental and control group outcomes. The column labeled "p" is the statistical significance level of the difference between experimental and control group outcomes: That is, p is the probability that average outcomes are different only because of random error. Statistical significance levels are indicated as *** = 1 percent; ** = 5 percent; * = 10 percent.

(b) Quarter 1 refers to the three calendar months beginning with the month in which the sample member was randomly assigned to the experimental or control group. Thus, e.g., for a young woman who was randomly assigned on May 16, 1990, quarter 1 means the period from May 1 through July 31, 1990.

TABLE 8.4

IMPACTS OF NEW CHANCE ON AVERAGE EARNINGS WITHIN 18 MONTHS AFTER RANDOM ASSIGNMENT, BY SUBGROUP

Characteristic and Subgroup at Random Assignment	Sample Size	Average Earnings, Quarters 1–6 ($) Experimentals	Controls	Within–Subgroup Impact	p (a)	Between–Subgroups Impact Difference (b)	p (a)
Age (years)						– –	0.327
16–17	408	1,445	1,333	111	0.741		
18–19	996	1,394	1,847	–452 **	0.031		
20–22	682	1,266	1,712	–445 *	0.080		
Ethnicity						– –	0.376
Black, non–Hispanic	1,093	1,334	1,522	–187	0.350		
Hispanic	465	1,359	1,701	–342	0.271		
White or other	527	1,431	2,115	–684 **	0.019		
Living arrangement						–81	0.794
Living with mother	710	1,073	1,483	–410	0.105		
Not living with mother	1,352	1,525	1,854	–329 *	0.069		
Number of children						118	0.696
1	1,356	1,424	1,724	–300 *	0.098		
More than 1	732	1,255	1,674	–418 *	0.086		
Age at first child's birth (years)						83	0.779
13–16	840	1,397	1,689	–291	0.208		
17–19	1,248	1,344	1,719	–375 **	0.045		
Age of youngest child (years)						175	0.551
Less than 1	1,119	1,398	1,665	–266	0.180		
1 or older	965	1,324	1,766	–441 **	0.040		
Educational attainment						347	0.570
No high school diploma or GED	1,952	1,300	1,626	–326 **	0.030		
Had a high school diploma or GED	132	2,326	2,999	–673	0.256		

(continued)

TABLE 8.4 (continued)

Characteristic and Subgroup at Random Assignment	Sample Size	Average Earnings, Quarters 1–6 ($)		Within–Subgroup Impact	p (a)	Between–Subgroups Impact Difference (b)	p (a)
		Experimentals	Controls				
Highest grade completed						–161	0.604
10th or below	1,384	1,281	1,684	–403 **	0.025		
11th or above	701	1,526	1,768	–242	0.337		
Interval since last attended regular high school						259	0.385
More than 2 years	1,097	1,495	1,748	–253	0.209		
2 years or less	934	1,213	1,726	–512 **	0.020		
TABE reading test score (grade equivalent) (c)						– –	0.683
Below 6th grade	431	965	1,233	–268	0.410		
6th or 7th grade	482	1,254	1,294	–40	0.896		
8th or 9th grade	584	1,684	2,181	–497 *	0.068		
10th grade or above	585	1,432	1,890	–458 *	0.095		
Ever employed						–190	0.595
Yes	1,640	1,483	1,854	–371 **	0.026		
No	448	954	1,135	–180	0.569		
Employed during prior year						–538 *	0.077
Yes	757	1,567	2,267	–699 ***	0.004		
No	1,323	1,249	1,410	–161	0.379		
Prior–year earnings						644 *	0.083
$0–$500	1,660	1,135	1,372	–237	0.146		
$501 or more	418	2,283	3,165	–881 ***	0.008		
Any AFDC received in household						173	0.793
Yes	1,976	1,300	1,628	–327 **	0.028		
No	109	2,579	3,080	–501	0.438		

(continued)

TABLE 8.4 (continued)

Characteristic and Subgroup at Random Assignment	Sample Size	Average Earnings, Quarters 1–6 ($) Experimentals	Controls	Within–Subgroup Impact	p (a)	Between–Subgroups Impact Difference (b)	p (a)
Family received AFDC when sample member was growing up						--	0.688
Always	344	1,152	1,225	−73	0.842		
Sometimes	977	1,331	1,670	−338	0.108		
Never	751	1,526	1,983	−457 *	0.063		
CES–D (depression) Scale (d)						--	0.445
0–15 (not at risk)	979	1,171	1,721	−550 **	0.011		
16–23 (at some risk)	539	1,542	1,705	−163	0.566		
24–60 (at high risk)	566	1,522	1,713	−191	0.497		
Sample size	2,088						

SOURCES: MDRC calculations from New Chance Enrollment Form and survey data.

NOTES: Calculations for this table used data for all 2,088 sample members for whom there were 18 months of follow–up survey data, including those with values of zero for outcomes and New Chance enrollees (i.e., experimentals) who did not participate in the program. The reported sample sizes may fall short of this number because of missing or unusable items from some sample members' questionnaires.

The averages are adjusted using a two–way analysis of covariance procedure controlling for up to 51 kinds of difference in characteristics, other than the characteristic used to define subgroups, before random assignment. The two categories used as factors were research status (i.e., membership in the experimental or control group) and, one at a time, the baseline characteristics indicated. Rounding may cause slight discrepancies in sums and differences.

(a) A two–tailed t–test was applied to each regression–adjusted within–subgroup impact and also, whenever there were two subgroups, to each difference between subgroup impacts. For each characteristic with more than two subgroups, an F–test was applied to the interaction between that characteristic and experimental or control status. The columns labeled "p" are the statistical significance levels of each within–subgroup impact and each between–subgroups difference in impacts: That is, p is the probability that sample estimates are different from zero or from each other only because of random error. Statistical significance levels are indicated as *** = 1 percent; ** = 5 percent; * = 10 percent.

(b) For each characteristic with only two subgroups, the between–subgroups impact difference is the impact for the first subgroup less the impact for the second subgroup. For characteristics with more than two subgroups, a between–subgroups impact difference cannot be calculated, as indicated by dashes in the table.

(c) The test used to measure reading ability was the reading part of the Tests of Adult Basic Education (TABE). Most sites administered the Survey Form of the test, but some administered the full reading test.

(d) The Center for Epidemiological Studies Depression (CES–D) Scale is a widely used measure of depression; scores can range from zero to 60.

scores) generally experienced smaller opportunity costs than women who did not have such barriers. Consequently, the more disadvantaged sample members experienced the smallest up-front earnings loss from their participation in New Chance.

This phenomenon is most visible in a comparison of subgroups based on baseline work experience. The differences in earnings impacts along dimensions of employment and earnings in the year before random assignment were large and statistically significant. Young women who had earned more than $500 in the year before entering the program earned almost $900 less than their control group counterparts during the follow-up period. Women who had earned from zero to $500 faced an average earnings loss of only $237 during the follow-up period. Thus, women in this latter group may be more likely to recover this initial opportunity cost and experience an eventual net gain from their participation in the program than women who had earned more in the year before entering New Chance, assuming that the program's eventual payoff is not substantially smaller for this lower-earning group.

C. Impacts on Earnings Across Sites

Table 8.5 shows the program's impact on 18-month earnings by site. Most sites registered negative impacts, which were large and significant in Allentown and Portland. However, the between-sites variation was not statistically significant, and apparent impact differences can be explained by differences in the characteristics of the sample members at the different sites. When differences in sample members' background characteristics were controlled for, the statistical significance of the negative impacts in Allentown and Portland disappeared.[6]

To examine the potential influence of welfare rules on the pattern of site impacts, the sites were subdivided into three groups by total grant level (the sum of AFDC and food stamps). There were no significant differences in earnings or welfare receipt across this dimension.

V. Job Characteristics, Job Tenure and Job Loss, and Efforts to Find Work

This section describes the early work experiences of New Chance sample members in greater detail. Since many measures and analyses presented here included only sample members who reported any employment during the follow-up period, no experimental impacts are presented and no statistical tests were performed.

A. Job Characteristics

The 18-month follow-up survey collected data on the current or most recent job held by sample members. These data indicated that young women who worked during the follow-up period reported having had many different types of jobs. In each research group, four jobs accounted for almost half of all reported jobs: cashier, child care provider, receptionist/secretary, and nurse's aide.[7] The only

---

[6] This adjustment was done by adding to the regression model interactions of sample members' baseline characteristics with the experimental dummy.

[7] The other half included many different job categories, ranging from social worker to taxicab driver.

TABLE 8.5

IMPACTS OF NEW CHANCE ON AVERAGE EARNINGS WITHIN 18 MONTHS AFTER RANDOM ASSIGNMENT, BY SITE

Site	Sample Size	Average Earnings, Quarters 1–6 ($)		Within–Site Impact	p (a)	Between–Sites Impact Difference p (a)
		Experimentals	Controls			
						0.219
Allentown	115	1,333	2,610	−1277 ** (b)	0.037	
Bronx	126	1,117	635	482	0.424	
Chicago Heights	69	3,021	2,181	840	0.298	
Chula Vista	127	773	1,677	−904	0.121	
Denver	110	1,411	1,053	358	0.569	
Detroit	169	1,415	1,766	−351	0.492	
Harlem	124	1,815	2,597	−782	0.200	
Inglewood	131	743	914	−171	0.767	
Jacksonville	144	1,717	1,958	−241	0.663	
Lexington	135	1,552	2,371	−819	0.163	
Minneapolis	121	1,675	1,815	−140	0.817	
Philadelphia	135	1,035	1,273	−238	0.674	
Pittsburgh	171	1,538	1,649	−111	0.826	
Portland	143	528	1,983	−1455 *** (b)	0.008	
Salem	134	2,102	1,295	807	0.163	
San Jose	134	841	1,660	−819	0.150	
Sample size	2,088					

SOURCES: MDRC calculations from New Chance Enrollment Form and survey data.

NOTES: Calculations for this table used data for all 2,088 sample members for whom there were 18 months of follow–up survey data, including those with values of zero for outcomes and New Chance enrollees (i.e., experimentals) who did not participate in the program.

The averages are adjusted using a two–way analysis of covariance procedure controlling for up to 36 kinds of difference in characteristics, other than site, before random assignment. The two categories used as factors were research status (i.e., membership in the experimental or control group) and site. Rounding may cause slight discrepancies in sums and differences.

(a) A two–tailed t–test was applied to each regression–adjusted within–site impact. An F–test was applied to the interaction between sites and experimental or control status. The columns showing p–values are the statistical significance levels of each within–site impact or between–sites impact difference: That is, p is the probability that sample estimates are different from zero or from each other only because of random error. Statistical significance levels are indicated as *** = 1 percent; ** = 5 percent; * = 10 percent.

(b) Impacts at these sites did not remain statistically significant after interactions between research status and the 36 non–site characteristics were added to the procedure.

apparent difference was that controls were more likely to have been cashiers (21.2 percent versus 17.8 percent for experimentals), while experimentals were more likely to have been office workers[8] (19.5 percent versus 13.5 percent for controls). These occupational differences should be interpreted with caution, since the differences were small, were based on small samples, and are nonexperimental.

Experimentals were probably more likely to have held a subsidized, paid internship, obtained with the help of the program. As discussed in Chapter 3, about 21 percent of experimentals were found to have had such a paid internship, which were of limited duration.[9] About 8 percent of all New Chance experimentals reported having found a job with the help of the program. While some of the jobs found through the program were work internships, others were permanent positions.

Type of work aside, New Chance does not appear to have affected the characteristics of sample members' jobs very much. Both experimentals and controls who worked reported having earned an average hourly wage of $4.90 in their last job, slightly more than the federal minimum wage of $4.25 in 1991. While experimentals who worked were more likely to have had medical benefits (16.8 percent versus 13.6 percent for controls), controls who worked were more likely to have had paid vacations (23.2 percent versus 20.2 percent for experimentals).

Overall, experimentals seemed more satisfied with their jobs than controls; on a 0 to 10 scale, they rated their satisfaction at 6.1, compared to 5.7 for controls. Similarly, workers in the experimental group expected greater opportunities for advancement than did controls, and felt that they had learned more on the job.

B. Job Tenure and Job Loss

Figure 8.1 shows the course of sample members' jobs in the first six months after they began. To ensure that the first six months would fall within the 18-month follow-up period, only jobs that started during the first 12 months after random assignment were included in Figure 8.1. It appears that roughly 10 percent of all these jobs ended in the first month, while roughly half (52.9 percent) ended within three months. At the end of six months, 27.9 percent were still ongoing. The figure also shows that experimentals and controls who worked during the first year of follow-up had similar job histories, with experimentals apparently having been somewhat more likely to lose their jobs in the first three months (47.7 percent versus 45.6 percent for controls), but also more likely to have held on to them beyond six months (29.0 percent versus 25.4 percent for controls).[10]

In the 18-month survey interview, sample members who had worked during the follow-up period, but who were not working at the time of the interview, were asked how their last job had

---

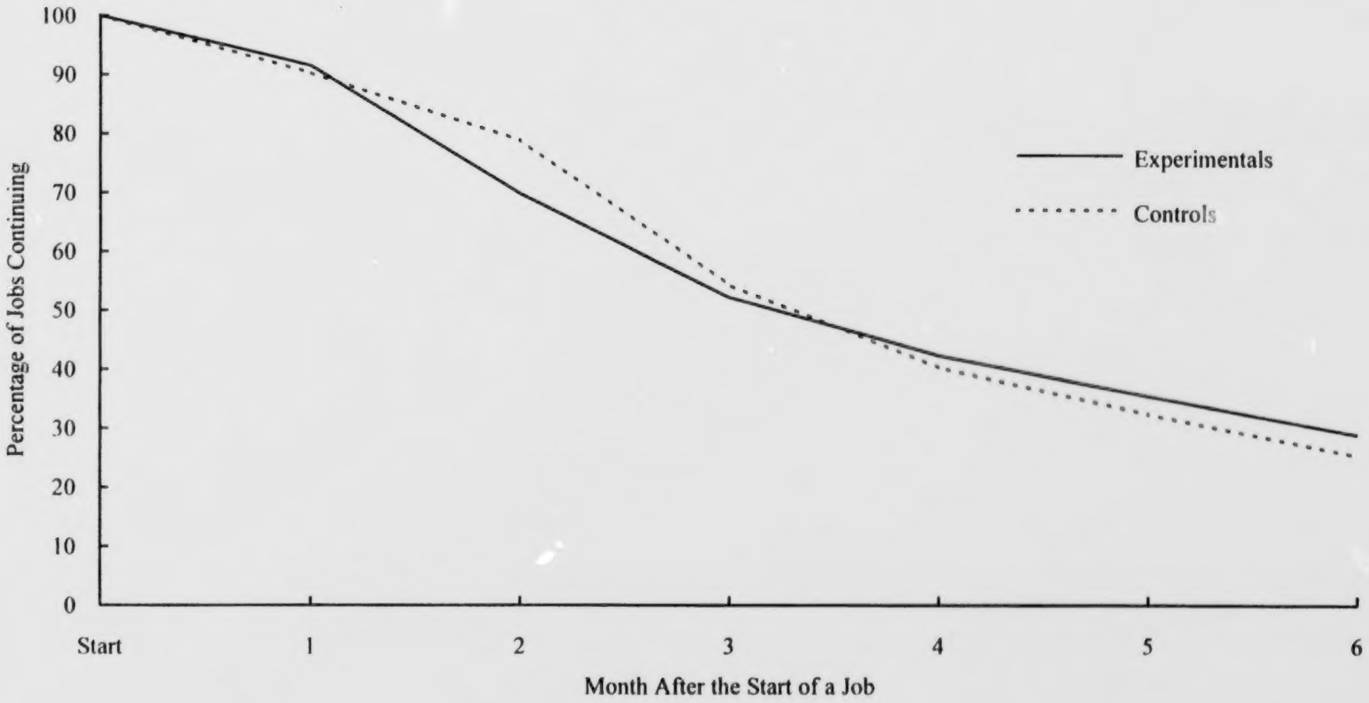
[8] Office work includes a broad range of occupations, such as secretary, typist, receptionist, file clerk, telephone operator, general office clerk, and data entry clerk.

[9] The follow-up survey registered such internships as regular paid employment. For control group members, the incidence of paid internships was not measured.

[10] Estimates of the average duration of all job spells were truncated at the end of the follow-up period, reducing the measured length of job spells. When the estimates were adjusted using a Tobit procedure, it was found that the average job would have lasted roughly five months (147 days for experimentals and 149 days for controls).

FIGURE 8.1

JOB TENURE AMONG EXPERIMENTALS AND CONTROLS, BY MONTH AFTER THE START OF A JOB



SOURCE: MDRC calculations from New Chance Enrollment Form and survey data.
NOTE: This graph includes jobs that began during the first 12 months of follow-up.

ended. From the answers, it appeared that most jobs (58.8 percent) had ended because sample members quit. Controls were significantly more likely to have quit a job than experimentals: 69.2 percent of the controls' jobs ended in this manner versus 53.9 percent of the experimentals' jobs ending.[11] Controls were also more likely to report that they were fired (7.5 percent of job terminations versus 4.5 percent for experimentals). Experimentals, on the other hand, were more likely to report the end of a temporary job as a reason for job termination (33.5 percent versus 15.0 percent for controls).[12] Approximately 8 percent of both groups reported being laid off.

Fifteen percent of those who had quit their job said they did so because they were unable to find good child care, and another 3.2 percent felt that they had to spend more time with their children; 10.2 percent quit because of a pregnancy; 17.7 percent did so because the job "did not pay enough" or had "insufficient or unpractical hours"; and 25.7 percent cited other job-related problems, such as problems with supervisors or "attitude" problems. Myriad other reasons were given for the remainder of "voluntary" terminations, including a return to school, medical issues, and safety.

There are many factors that could explain why women decided to leave a job within several months after they entered it. Often, the dynamics of the welfare system itself appear to affect these decisions: when a young woman on welfare first enters a new job, she experiences a brief period during which her income increases substantially. During this period of several months, reductions in the young woman's welfare grant are limited by earnings disregard rules and delayed because of processing time. While the young women enjoy an initial increase in their disposable incomes, these welfare rules catch up with them, resulting in substantial reductions in income after several months. This, in turn, may reduce the women's enthusiasm about their jobs and may force them to quit if their earnings are insufficient to offset the loss of welfare income and job-related expenses. These financial disincentives are exacerbated if reimbursable work expenses, such as child care, have to be paid out-of-pocket first.

C. Efforts to Find Work

At the end of the follow-up period, 83.7 percent of experimentals and 81.7 percent of controls were not working (a difference that was not statistically significant). More than a third of these nonworkers reported that they were looking for work (38.0 percent of experimentals and 38.8 percent of controls), with many of those who were looking for work reporting that they had applied for jobs directly with employers during the four weeks before the interview (63.8 and 61.2 percent of experimentals and controls, respectively). Almost half of both groups (47.1 percent of experimentals and 47.0 percent of controls) reported having looked at newspaper ads during this period, but only 14.9 percent of experimentals and 15.3 percent of controls actually answered any.

When those who were *not* looking for a job were asked why, they gave as their main reason: lack of education or training (the response of 19.8 percent of experimentals and 25.4 percent of controls), a preference for staying at home with their children (19.8 percent of experimentals and 15.8

---

[11] This comparison was limited to those who reported a job spell having ended and may therefore have been subject to selection bias.

[12] This finding may be related to the fact that many experimentals had paid work internships, which were temporary by design.

Problems at Work (and at Home)

The following profile of "Dolores," taken from the New Chance monograph (Quint and Musick, 1994), illustrates how a variety of personal difficulties – some of her own making, some not – could interfere with workplace performance and lead to termination of her job. Although Dolores was working, she had been given a final warning and had been suspended from work for a week because of excessive lateness.

Dolores, her two children, and her boyfriend of three years, Tony, live in a pleasantly furnished duplex apartment in a public housing project lost in the hills of the city where she lives and far from public transportation and other services; the area is one of high crime and vandalism. Dolores is a nurse's aide in a nursing home. In nice weather, she enjoys walking to work; if she is out the door at 6:30 A.M., she gets to work at 7 A.M., just in time for the start of her shift. In cold weather, she takes a cab; the driver picks her up at 6:45 A.M. and charges her $3 each way for the trip. Since her children do not have to leave for school until 7:30 A.M., Tony sees them off in the morning.

Tony is a drug dealer and has, for much of the time Dolores has known him, been in and out of jail. During his most recent spell in jail, she stayed home to see her children off and to lock up after them. She repeatedly called her supervisor to report that she could not come in before 7:30 A.M.; hence, her one-week suspension. Dolores explained that the rough neighborhood she lives in makes it essential that she lock the door carefully.

> This door right here, that's boarded up? They [her neighbors] kicked her door in, 'cause she went out of town. She ain't do nothing to nobody, but they just kicked her door 'cause she went out of town . . . I ain't never done nothing to nobody, but my door's kind of hard to lock. And if you don't turn it the right way, you hear a click, but that don't mean it's locked.

It was not ascertained whether Dolores has explored other options that would enable her to get to work on time: replacing the lock or finding a neighbor with whom she can leave the children until it is time for them to go to school. Ending her relationship with Tony might simplify her life, but this she is unwilling to do. Although she deplores the way he makes a living, she sees him as generous and dependable. And she has been with him longer than with any other man.

Dolores resents her week's suspension and seems to think that her supervisor should excuse her lateness because she believes she had a good reason for it. She is also especially indignant because she sees nurses's aides and nurses being treated very differently for the same behavior:

> The head of the place, you know, it's like her way or no way. She'll get on us about the aides coming in late and what we're doing, but a nurse could come in every single day late, and you say, "Okay, well, if you're going to bawl me out about being late, what about her?" "Oh, don't worry about her, worry about yourself." And I think that is very, very, very unfair, you know? Very unfair.

Dolores' bitterness toward her supervisor may be detrimental to her ability to keep her job. At the same time, the objective reality of her situation forces her to confront a problem with which a working mother living in a safer neighborhood would not have to cope.

percent of controls), current participation in education or training activities (17.7 percent of experimentals and 13.2 percent of controls), or pregnancy (12.8 percent of experimentals and 14.9 percent of controls). While experimentals were more likely not to be looking for work because they were in an education or training activity, controls were more likely to mention a *need* for education or training as their main reason for not looking for work. Interestingly, only a handful of respondents claimed that "no work was available."

VI. Impacts on Welfare Receipt and Family Income

Upon their entry into the study, most sample members (94.8 percent) were receiving some form of public assistance, mostly AFDC and food stamps. Throughout the follow-up period, this income was supplemented with the young women's earnings or the earnings of their partners. However, despite increasing earnings, it appears that AFDC remained the major measured source of cash income throughout the follow-up period for both experimentals and controls.[13]

A. Aggregate Impacts on Welfare Receipt

While the program was supposed to increase earnings and reduce long-term AFDC receipt, no short-term reductions were expected. As shown in Table 8.6, overall, there were no program impacts on AFDC receipt, though one quarterly impact was statistically significant. Control group levels of AFDC receipt declined slowly over time from 94.8 percent in the first three months after random assignment to 87.6 percent in the final three months before the interview. Experimentals reduced their average rate of AFDC receipt as well, but with some delay, resulting in a significant positive program impact on (i.e., an increase in) AFDC receipt in the second quarter after random assignment.

As the table also shows, the program had no significant impact on number of months of AFDC receipt or on the young women's AFDC status in the month before the interview, when over 80 percent of both groups were still on welfare. Also, as shown in Table 8.7, the average AFDC grant in the month before the interview was very similar for both research groups: $324 for experimentals and $319 for controls. The program does seem to have increased experimentals' exposure to sanctions (i.e., reductions in the AFDC grant). Experimentals were significantly more likely to have been sanctioned at some point during the follow-up period for failure to attend a required education or work program. When such a sanction occurred, it was also likely to last longer for experimentals who were sanctioned (an average of 3.2 months versus 2.0 months for controls who were sanctioned).

B. Impacts on Work and Welfare Dynamics

Table 8.6 shows that sample members were receiving AFDC for an average of 16.1 months in the 18-month follow-up period. There was no statistically significant difference between the number of months experimentals and controls received AFDC. In line with this average, only 20.9 percent of all sample members reported ever having left AFDC during the 18 months of follow-up and, of those who left, 44.2 percent returned before the follow-up interview. About one-third of welfare exits (30.7 percent) were accompanied by employment in the prior month, making it likely that these were work-related exits. Other welfare spells may have ended for other reasons, such as a change in living arrangements, a move, marriage, or failure to have AFDC eligibility recertified. The average

---

[13] Earnings of household members other than the young woman's partner or husband were not measured.

TABLE 8.6

IMPACTS OF NEW CHANCE ON AFDC RECEIPT WITHIN 18 MONTHS AFTER RANDOM ASSIGNMENT

Outcome and Follow-Up Period	Experimentals	Controls	Difference	p (a)
Ever received AFDC (%)				
Quarter 1 (b)	95.7	94.8	1.0	0.253
Quarter 2	93.9	91.0	2.9 ***	0.008
Quarter 3	91.2	89.2	2.0	0.122
Quarter 4	89.8	88.7	1.0	0.450
Quarter 5	88.3	88.6	−0.3	0.821
Quarter 6	88.6	87.6	1.0	0.488
Ever received AFDC, quarters 1−6 (%)	98.0	97.6	0.5	0.452
Average number of months of AFDC receipt	16.1	15.9	0.2	0.239
Receiving AFDC at 18 months (%)	82.1	81.5	0.7	0.699
Ever sanctioned (i.e., had AFDC grant cut) for failure to attend a required education or work program (%)	5.0	3.0	2.0 **	0.044
Sample size	1,408	680		

SOURCES: MDRC calculations from New Chance Enrollment Form and survey data.

NOTES: Calculations for this table used data for all 2,088 sample members for whom there were 18 months of follow-up survey data, including those with values of zero for outcomes and New Chance enrollees (i.e., experimentals) who did not participate in the program.

The averages or percentages are adjusted using linear analysis of covariance procedures controlling for up to 51 kinds of difference in characteristics before random assignment. Rounding may cause slight discrepancies in sums and differences.

(a) A two-tailed t-test was applied to each regression-adjusted difference between average experimental and control group outcomes. The column labeled "p" is the statistical significance level of the difference between experimental and control group outcomes: That is, p is the probability that average outcomes are different only because of random error. Statistical significance levels are indicated as *** = 1 percent; ** = 5 percent; * = 10 percent.

(b) Quarter 1 refers to the three calendar months beginning with the month in which the sample member was randomly assigned to the experimental or control group. Thus, e.g., for a young woman who was randomly assigned on May 16, 1990, quarter 1 means the period from May 1 through July 31, 1990.

measured duration of all welfare exits was 6.3 months. However, this duration may actually have been longer, since at follow-up 18.1 percent of sample members were not receiving AFDC, and the length of their period off the rolls could not be measured.

Since almost half of all experimentals and controls were working at some point during the follow-up (see Table 8.1), and only 20.9 percent had ever left welfare, most sample members who were working apparently continued to receive welfare benefits while they were working (not shown in the table). More than one-third of all sample members combined work and welfare in some month during the follow-up period (36.7 percent of experimentals and 37.5 percent of controls, not shown in tables). Those who combined work and welfare did so for about five months on average (4.6 months for experimentals, and 5.1 months for controls, not shown in tables). For many young women in New Chance who worked, continuing receipt of welfare was necessary to secure enough income to sustain their families. As a result, young women in New Chance may have actively avoided having their welfare case closed, for fear of having to wait several months before being able to get back on welfare if a job would end. Also, some New Chance coordinators reported that combining work and welfare could be quite difficult within existing welfare rules. In some situations child care benefits, e.g., were much more difficult to secure for women who combined work with welfare than for those who left the rolls.

C. Aggregate Impacts on Family Income

Table 8.7 combines different income sources reported at the time of the 18-month interview. The top panel shows the percentage of all experimentals and controls who reported having a particular source of income. The bottom panel shows the contribution of these income sources to total family income in the month before the interview.[14] It appears that earnings from a husband or partner were an important source of income for young women who had access to it. While only 13.5 percent of control group members reported this income source, the mean dollar value was greater than that of the young women's own earnings (which 21.7 percent of controls reported). The average earnings for women who worked in the month before the interview were $608, while the average earnings of a working partner were $1,037 (not shown in the table). For sample members who either worked or had husbands or partners who worked in the month before the interview, all combined income sources added up to an average of $1,302 in that month (not shown in the table). Some further analysis was done to determine how household income for workers compared to federal poverty standards. For each household[15] with a working member, total measured income was compared to the 1993 federal poverty threshold. It was found that 60.3 percent of all households with a working member would have had annual incomes exceeding the poverty level for their size if their reported monthly income prior to the interview were to be sustained for a full year.[16]

---

[14] For this measure, "family income" was defined as the income of the sample member and that of her husband or partner. Income of other household members was excluded because it was believed that the sample member would not have had control over such income.

[15] For this purpose "household" was defined as the sample member, a husband or partner (if one was present) and any children.

[16] In a comparable study of teenage parents, Maynard, Nicholson, and Rangarajan (1993), using similar computations, found that the household income of 79 percent of a sample of teenage parents who received enhanced services in the Teenage Parent Demonstration and who reported working would have exceeded the poverty level if sustained for a year. However, these calculations do not include earnings from a husband or partner, except for formal and informal child support.

TABLE 8.7

SELECTED IMPACTS OF NEW CHANCE ON INCOME AT 18 MONTHS AFTER RANDOM ASSIGNMENT

Outcome	Experimentals	Controls	Difference	p (a)
Incidence of selected sources of income during the month prior to the survey interview (%)				
Sample member's employment	20.2	21.7	-1.5	0.430
Husband's or partner's employment	15.4	13.5	1.9	0.238
Sample member's AFDC case	82.1	81.5	0.7	0.699
Husband's or partner's AFDC case	0.9	1.0	-0.1	0.792
Food stamps	86.2	85.4	0.9	0.584
Supplemental Security Income (SSI)	2.5	1.7	0.9	0.204
Unemployment or worker's compensation	1.2	1.3	-0.1	0.873
Alimony or child support	11.7	11.6	0.1	0.943
Private or government pension	0.8	0.7	0.0	0.909
Family or friends	11.5	11.0	0.5	0.734
Public housing or rent assistance	34.8	30.4	4.4 **	0.024
Average measured income from selected sources during the month prior to the survey interview ($)				
Sample member's earnings	105	132	-27 **	0.045
Husband's or partner's earnings	161	140	20	0.322
Sample member's AFDC grant	324	319	5	0.553
Husband's or partner's AFDC grant	2	2	0	0.877
Food stamps	163	165	-2	0.673
Supplemental Security Income (SSI)	11	8	3	0.342
Unemployment or worker's compensation	5	5	0	0.979
Alimony or child support	11	12	0	0.921
Private or government pension	3	2	1	0.466
Family or friends	19	15	3	0.356
Average total measured income in the month prior to the survey interview ($)	802	799	3	0.905
Sample size	1,408	680		

SOURCES: MDRC calculations from New Chance Enrollment Form and survey data.

NOTES: Calculations for this table used data for all 2,088 sample members for whom there were 18 months of follow-up survey data, including those with values of zero for outcomes and New Chance enrollees (i.e., experimentals) who did not participate in the program.

The averages or percentages are adjusted using linear analysis of covariance procedures controlling for up to 51 kinds of difference in characteristics before random assignment. Rounding may cause slight discrepancies in sums and differences.

(a) A two-tailed t-test was applied to each regression-adjusted difference between average experimental and control group outcomes. The column labeled "p" is the statistical significance level of the difference between experimental and control group outcomes: That is, p is the probability that average outcomes are different only because of random error. Statistical significance levels are indicated as *** = 1 percent; ** = 5 percent; * = 10 percent.

From Table 8.7, it is also apparent how dependent these young women were on AFDC and food stamp income. More than four out of five controls reported having received income from these sources, which together made up more than 60 percent of the average control group member's measured budget at follow-up.

Program impacts on total family income and on the relative contribution of different income sources were limited. Experimentals reported significantly less income from their own earnings than controls (an average loss of $27.03 in the month before the follow-up interview), but this difference was not reflected in their total measured income for the month: Both experimentals and controls reported average total family income of around $800. The negative program impact on individual earnings was offset by larger contributions of husbands and partners ($20.11), and marginally larger contributions from AFDC and Supplemental Security Income (SSI): $4.51 and $2.87, respectively.[17] Only the negative impact on individual earnings was statistically significant.[18]

The program also significantly affected a non-cash contribution to family welfare: access to public housing and rent assistance. At certain sites, with the help of case managers and program operators, experimentals were more successful than controls in securing these housing services; the program significantly increased access to public housing and housing subsidies from 30.4 percent to 34.8 percent. Some New Chance sites helped successful participants gain access to Section 8 housing subsidies.

This analysis of welfare receipt and family income shows that the program had little impact on these measures in the first 18 months of follow-up. In the early part of this follow-up period, experimentals were somewhat less likely to leave welfare for work, but this difference was small. By the end of the follow-up period, the negative earnings gap largely disappeared, and differences in welfare receipt were not statistically significant.

VII. Impacts on Skill-Building Activity

During the early follow-up period covered by this report, New Chance participants spent more time in education and training and less time in the labor market than young women in the control group, leading to modest but significant opportunity costs in terms of lost earnings. However, the question remains whether the program just replaced some work activity with education and training or actually increased participants' combined employment and education and training efforts. This final section explores this issue in further detail by examining a composite measure of "skill-building activity."

For this purpose "skill-building activity" is defined as either work, participation in an education or training program, or participation in unpaid work experience. Table 8.8 and Figure 8.2 show how these measures developed over time for experimentals and controls. In the first year after random assignment, the program significantly increased experimentals' level of skill-building activity relative to controls. While 78.9 percent of experimentals were in such activities in the first three months after

---

[17] Supplemental Security Income (SSI) is a form of federally assisted cash assistance, provided to those who are aged, disabled, or blind.

[18] These income measures were collected only for the last month before the follow-up interview. As a result, they are not as stable or reliable as other outcomes presented in this chapter.

TABLE 8.8

IMPACTS OF NEW CHANCE ON SKILL-BUILDING ACTIVITY WITHIN 18 MONTHS AFTER RANDOM ASSIGNMENT

Outcome and Follow-Up Period	Experimentals (%)	Controls (%)	Difference	p (a)
Employed or in education or training or work internship				
Quarter 1 (b)	78.9	36.1	42.8 ***	0.000
Quarter 2	74.0	47.9	26.1 ***	0.000
Quarter 3	63.6	47.7	15.9 ***	0.000
Quarter 4	54.8	49.1	5.7 **	0.013
Quarter 5	54.1	50.1	4.0 *	0.083
Quarter 6	54.5	52.8	1.7	0.463
Quarters 1-4	90.6	69.7	20.9 ***	0.000
Ever employed or in education or training or work internship				
Quarters 1-6	94.0	82.0	12.1 ***	0.000
Employed or in education or training or work internship at 18 months	34.8	36.9	-2.1	0.345
Sample size	1,408	680		

SOURCES: MDRC calculations from New Chance Enrollment Form and survey data.

NOTES: Calculations for this table used data for all 2,088 sample members for whom there were 18 months of follow-up survey data, including those with values of zero for outcomes and New Chance enrollees (i.e., experimentals) who did not participate in the program.

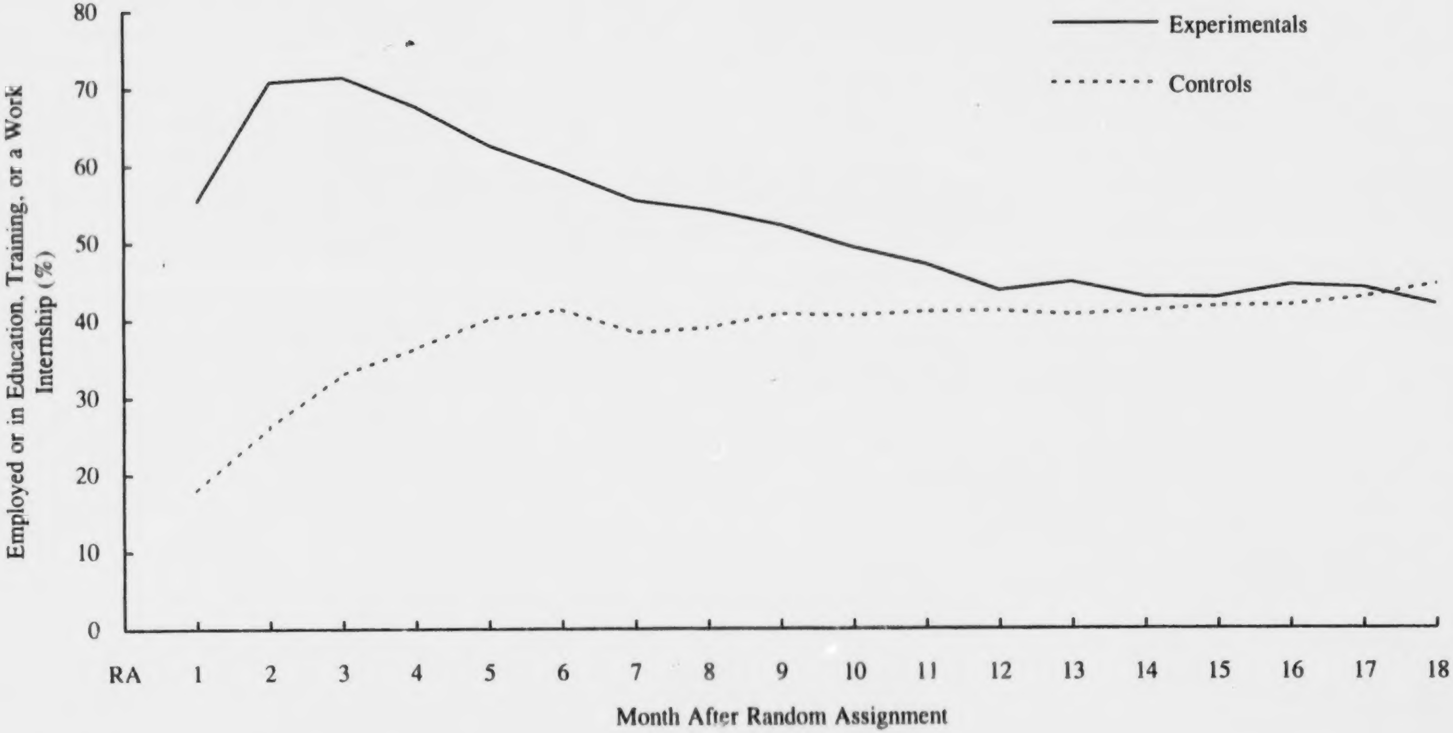
The percentages are adjusted using linear analysis of covariance procedures controlling for up to 51 kinds of difference in characteristics before random assignment. Rounding may cause slight discrepancies in sums and differences.

(a) A two-tailed t-test was applied to each regression-adjusted difference between average experimental and control group outcomes. The column labeled "p" is the statistical significance level of the difference between experimental and control group outcomes: That is, p is the probability that average outcomes are different only because of random error. Statistical significance levels are indicated as *** = 1 percent; ** = 5 percent; * = 10 percent.

(b) Quarter 1 refers to the three calendar months beginning with the month in which the sample member was randomly assigned to the experimental or control group. Thus, e.g., for a young woman who was randomly assigned on May 16, 1990, quarter 1 means the period from May 1 through July 31, 1990.

FIGURE 8.2

PERCENTAGE OF EXPERIMENTALS AND CONTROLS EMPLOYED OR IN EDUCATION, TRAINING, OR A WORK INTERNSHIP, BY MONTH AFTER RANDOM ASSIGNMENT



SOURCE: MDRC calculations from survey data.

random assignment, only 36.1 percent of controls were, for a significant difference of 42.8 percentage points. However, this difference became smaller over time as experimentals left New Chance, and it appears that controls caught up with experimentals by the time of the interview. This was unexpected because, even if control group members found education and training activities on their own during the follow-up period, experimentals were anticipated to increase their work effort and thus to remain ahead in overall skill-building activity.

There are several possible explanations for these results. Experimentals may have experienced a lag between their completion of education and training activities and their subsequent entry into the workforce. A similar lag was identified in the post-program experiences of Job Corps participants (Mallar, Thornton, and Long, 1982).

Another, more pessimistic, possible explanation is that the program's initial "push" was not strong enough to give participants a long-term edge over control group members. At this point, it is unclear what will prove to be the correct explanation. Any delayed effects on earnings and self-sufficiency will appear in the final report, based on 42 months of follow-up.

VIII. New Chance and Other Programs Compared

Several other recent studies have examined the effects of education and training programs on the labor market and welfare experiences of young mothers. As discussed in earlier chapters, two evaluations are of special interest in this regard, the JOBSTART Demonstration and the Teenage Parent Demonstration. In both studies, a subgroup could be selected from the analysis sample that is comparable to the New Chance sample featured in this report, thus allowing for a comparison of outcomes and program impacts across these studies.

Table 8.9 compares some key labor market and welfare outcomes for the New Chance, Teenage Parent Demonstration, and JOBSTART samples. The table shows that the early earnings impacts from JOBSTART were more favorable than those from New Chance. Apparently, the opportunity costs of participating in New Chance were greater than they were in JOBSTART, even though New Chance controls did not earn as much as JOBSTART controls during the first 18 months of follow-up.

The table also shows that the Teenage Parent Demonstration was more successful in reducing the amount of welfare received at the 18-month point. Part of that may be attributable to its mandatory character, which may have given some of those assigned to the program (including those who did not participate) a greater incentive to find work and leave welfare on their own. The program's mandatory nature also increased participants' exposure to sanctions, which may have accounted for a substantial proportion of the welfare savings.

TABLE 8.9

A COMPARISON OF IMPACTS ON EMPLOYMENT, EARNINGS, AND WELFARE RECEIPT IN NEW CHANCE AND SELECTED OTHER DEMONSTRATIONS FOR YOUNG MOTHERS

	New Chance			JOBSTART			Teenage Parent Demonstration		
Outcome	Experimentals	Controls	Difference	Experimentals	Controls	Difference	Experimentals	Controls	Difference
Ever employed (%)	42.7	44.9	−2.2	55.5	50.8	4.7	42.2	36.7	5.6 (a)
Average earnings in months 1−18 ($)	1,366	1,708	−342 **	2,052	1,992	60	N/A (b)	N/A	N/A
Average number of months on AFDC	16.3	16.1	0.2	9.9	10.0	−0.1	N/A	N/A	N/A
AFDC amount at 18 months ($)	323.50	319.00	4.50	191.00	189.00	2.00	224.6	241.96	−17.39 (a)

SOURCES: MDRC calculations from New Chance Enrollment Form and survey data; Cave and Doolittle, 1991; Maynard, Nicholson, and Rangarajan, 1993.

NOTES: A two−tailed t−test was applied to regression−adjusted differences between the experimental and control groups. Statistical significance levels are indicated as *** = 1 percent; ** = 5 percent; * = 10 percent.

(a) The aggregate numbers presented here were calculated based on information on site−specific numbers in the three demonstration sites; significance levels for the pooled sample are not available. However, at 18 months after random assignment, the experimentals had a significantly higher rate of employment in one site (Chicago), and received a significantly lower amount of AFDC in all three sites.

(b) N/A indicates that the specified data item was not available.

CHAPTER 9

SHORT-TERM ANSWERS, LONG-TERM QUESTIONS

I. Introduction

This report is only the first installment of the New Chance impact story. At this point, the story offers few definitive answers and leaves many open questions. Overall, there are three paramount questions, one particular to New Chance and the others broader in scope. (1) What do these early findings suggest about the longer-term prospects for the young mothers and their children? (2) What is the significance of the findings for the creation of effective public policy for young mothers receiving AFDC? (3) What are the larger implications of the findings for the design and operation of programs aimed at improving the human capital and personal development of disadvantaged young people, especially those who are parents? This chapter proposes answers to many of these questions, sometimes tentative, sometimes based on the New Chance sites' own experiences and the changes they have made over time. Equally important, it seeks to bring the questions themselves into the foreground of discussion.

The next section grounds the discussion by recapitulating the report's main findings and by bringing together data from the various chapters to address cross-cutting issues, including effects for particular subgroups and sites. The third section conjectures about the meaning of these findings for the future well-being of the young mothers and their children, especially given their changing and uneven life courses. The chapter concludes by considering the implications of these data (and, where relevant, the findings of other demonstrations serving young mothers) for policy formulation and program design. Welfare reform proposals have largely centered on mandatory participation in education and work programs; New Chance was, in the main, voluntary. Nonetheless, the New Chance results suggest the numerous complexities that arise in putting in place programs for young mothers, whether those programs be voluntary or mandatory.

II. The Findings in Summary

A. Findings on Implementation

Each of the Phase I services was received by the majority of enrollees, and enrollees rated these services high. Absenteeism was frequent at the majority of sites, however, in part because site staff initially failed to enunciate and enforce expectations and rules, and in part because participants experienced many personal and situational difficulties or lacked sufficient motivation.

The education, parenting, and life skills components were notably strong at most sites. But case managers did not consistently follow up on the young women's family planning practices in individual counseling sessions. The employability development component was generally the weakest of the Phase I components; sites often had only limited experience with this component, and, in general, enrollees were more motivated to attain a GED than to enter skills training or jobs.

Largely because of early departures from the program, fewer than half of all enrollees entered Phase II components. Rising caseloads also meant that case managers generally had difficulty maintaining frequent contact with the young women who did enter these components, especially if, as was usually the case, these services were off-site.

B. Findings on Impacts

Program impacts are summarized in Table 9.1. In examining these results, it is important to remember that the New Chance Demonstration measures the effects of a particular package and dosage of services, over and above what control group members – who, like experimentals, volunteered for New Chance and might therefore have been expected to seek out other services – assembled on their own. Indeed, substantial numbers of controls did pursue other education services.[1] Nonetheless, experimentals were significantly more likely than controls to have received every kind of service offered; this difference was especially large with respect to personal development and parenting classes.

The fact that experimentals received more, and more varied, services than did controls points to the advantages of a "one-stop shopping" approach, in which comprehensive services are delivered at a single location. Yet, the New Chance experience also indicates that such an approach is not a panacea. It may facilitate service receipt, but it does not ensure good attendance or high retention.

The program had a substantial and positive impact on GED attainment that was statistically significant across almost all subgroups of the population (the chief exception having been those sample members who at random assignment read below the sixth-grade level). The program's strong impact on GED receipt is also of interest because, New Chance staff report, the desire to obtain a GED was the main factor propelling enrollees to join the program. However, there was no concomitant impact on reading scores.

Roughly equal percentages of young mothers in New Chance and in the control group gave birth during the 18-month follow-up period. But a significantly higher proportion of experimentals than controls reported a pregnancy, and a significantly higher proportion reported an abortion as well. Furthermore, among sample members who reported at the 18-month interview that they were currently sexually active, experimentals were significantly less likely to be using contraception regularly than their control counterparts.

At follow-up, experimentals were significantly more likely than controls to be living with a partner or husband and less likely to be living with a parent or grandparent. Importantly, the program's impact on subsequent pregnancies was concentrated among those young mothers who were living with a partner or husband at follow-up.[2]

With regard to child-related outcomes, the biggest disparity was in the area of child care: Children of experimentals were in non-maternal care for longer periods of time, were more likely to

---

[1] New Chance controls were more likely to have participated in education programs than were experimentals in both the Teenage Parent Demonstration and LEAP, two mandatory programs for young mothers.

[2] It is also worth recalling that the program's impact on GED receipt was not restricted to those who managed to avoid a pregnancy: A significantly higher proportion of experimentals than controls both experienced a subsequent pregnancy *and* earned a GED.

TABLE 9.1

SELECTED IMPACTS OF NEW CHANCE AT OR WITHIN 18 MONTHS AFTER RANDOM ASSIGNMENT

Outcome	Experimentals	Controls	Difference
Service receipt			
Ever participated in (%)			
Basic education/GED	85.3	60.4	24.9 ***
Skills training	33.3	22.5	10.8 ***
Parenting classes	66.5	20.6	45.9 ***
Family planning classes	51.7	11.9	39.8 ***
Life skills classes	51.6	12.4	39.2 ***
Health education classes	49.3	11.0	38.3 ***
Educational attainment and achievement			
Education credentials at month 18 (%)			
High school diploma or GED	43.1	30.0	13.1 ***
Credits toward A.A. or B.A. degree	9.8	7.1	2.6 **
Trade certificate or license	12.5	12.4	0.1
Average TABE reading score at follow-up	748.7 (a)	748.3 (a)	0.4
Fertility-related behavior			
Ever gave birth during months 1-18 (%)	28.4	26.2	2.2
Ever became pregnant during months 1-18 (%)	57.0	53.0	4.0 *
Ever had an abortion during months 1-18 (%)	14.9	11.1	3.8 **
Sexually active, not contracepting regularly at follow-up (b) (%)	30.2	25.2	4.9 **
Living arrangement at follow-up			
Living with parent or grandparent (c) (%)	28.2	34.8	-6.5 ***
Living with husband or partner, but without parent or grandparent (%)	22.8	19.6	3.2 *
Living with child(ren) only (%)	35.8	33.9	2.0
Living alone or other (%)	13.1	11.8	1.3
Child-related outcomes			
Ever in any regular child care arrangement (d, e) (%)	95.3	85.3	10.1 ***
Average number of months in a regular child care arrangement (d)	10.6	8.3	2.4 ***
Ever in a day care center or preschool (%) (d)	63.4	33.4	30.0 ***
Ever in a regular child care arrangement before age 1 (d) (%)	48.4	41.0	7.4 ***
In a preschool/child care arrangement at follow-up (d) (%)	50.7	53.7	-3.0
Average score on HOME scale (f)	100.5	100.3	0.2
Average score on Emotional Support subscale of HOME	100.6	99.3	1.3 *

(continued)

TABLE 9.1 (continued)

Outcome	Experimentals	Controls	Difference
Employment and welfare receipt			
Ever employed (%)			
Months 1–3	8.0	12.7	-4.7 ***
Months 4–6	14.4	19.2	-4.8 ***
Months 7–9	17.9	20.3	-2.4
Months 10–12	18.5	21.2	-2.7
Months 13–15	22.3	23.8	-1.5
Months 16–18	26.8	26.3	0.4
Months 1–18	42.6	44.9	-2.2
Ever received AFDC (%)			
Months 1–3	95.7	94.8	1.0
Months 4–6	93.9	91.0	2.9 ***
Months 7–9	91.2	89.2	2.0
Months 10–12	89.8	88.7	1.0
Months 13–15	88.3	88.6	-0.3
Months 16–18	88.6	87.6	1.0
Receiving AFDC at 18 months (%)	82.1	81.5	0.7
Other areas			
At risk of clinical depression at follow–up (g) (%)	45.2	44.2	0.9
Reported no one available as a social support at follow–up (%)	5.4	8.1	-2.7 **
Sample size	1,408	680	

SOURCES: MDRC calculations from New Chance Enrollment Form and survey data.

NOTES: Calculations for this table used data for all 2,088 sample members for whom there were 18 months of follow–up survey data, including those with values of zero for outcomes and New Chance enrollees (i.e., experimentals) who did not participate in the program.

The averages or percentages are adjusted using a two–way analysis of covariance procedure controlling for up to 51 kinds of difference in characteristics, other than the characteristic used to define subgroups, before random assignment. The two categories used as factors were research status (i.e., membership in the experimental or control group) and, one at a time, the baseline characteristics indicated. Rounding may cause slight discrepancies in sums and differences.

A two–tailed t–test was applied to each regression–adjusted within–subgroup impact. Statistical significance levels are indicated as *** = 1 percent; ** = 5 percent; * = 10 percent.

(a) The test administered was the reading part of the Tests of Adult Basic Education (TABE), Survey Form, a 30–item test of reading vocabulary and reading comprehension. These TABE reading scores both are equivalent to a reading grade level of 7.8.

(b) A respondent who reported using contraception at each intercourse and/or who said that she always took a birth control pill when she was supposed to was considered to be contracepting regularly.

(c) Using a multinomial logit estimator, the distributions for experimentals and controls in these mutually exclusive categories of living arrangements were compared and found to be statistically significantly different from one another.

(d) Regular child care was defined as an ongoing arrangement used while the mother was in school, in training, or working.

(e) The child pertains to a focal child, who was randomly selected from among children already born at baseline.

(f) A modified version of the short form of the Home Observation for Measurement of the Environment (HOME) Scale (first administered in the National Longitudinal Survey of Youth) was administered. Scores here were age–standardized to have a mean of 100 and a standard deviation of 15.

(g) Those with scores below 16 on the CES–D are considered not to be at risk of depression; those with scores of 16 and above are considered at risk.

have been cared for in day care centers or preschools, and were more likely to have entered a nonmaternal child care arrangement before they were a year old. Differences in child care arrangements had disappeared by the follow-up interview, however. The quality of care delivered by New Chance on-site child care centers generally met or surpassed experts' standards for good-quality care and was substantially better than the care typically delivered by centers serving low-income children. Children of both experimentals and controls were being raised in environments that were, overall, quite similar, although mothers in the experimental group provided more emotional support to their children and expressed less authoritarian parenting attitudes.

Overall, 43 percent of the experimentals and 45 percent of the controls – a nonsignificant difference – worked at some point during the follow-up period. Experimentals were somewhat less likely than controls to have been employed during the first 12 months of the follow-up period, but by the last two quarters of follow-up, employment rate differences between the two groups were no longer statistically significant. Despite the controls' higher employment and earnings early on, experimentals had a significantly higher AFDC receipt rate during only one of the six quarters of follow-up.

Impacts in other areas were small or nonexistent. The program did not measurably affect either maternal or child health. Nearly half of the women in both groups were judged to be at risk of clinical depression; there was no difference between the groups in this regard. Women in the experimental group, however, were significantly less likely to report that they had no one to turn to for emotional support, and they were more satisfied with the support they received.

Thus, while the findings of this report are mixed, in the main, they are less positive than had been hoped for at this early point. The program generated positive impacts in some areas, but in other areas, effects either were not found or were in the opposite direction of what had been hypothesized.

C. Findings on Impacts for Subgroups and Sites

Sometimes program effects that are quite small overall mask considerably larger impacts for particular subgroups of enrollees or for specific sites. Program impacts on subgroups are of interest because they may help planners target programs toward those who will especially benefit from them – or decide not to target. Also, sites that have positive impacts across a range of areas may constitute useful models for other sites to follow.

New Chance, by design, is targeted toward welfare mothers who are young and who are high school dropouts. In general, the subgroup data suggest that more refined targeting is not warranted. In some outcome areas, impacts were found across virtually all subgroups: The subgroup effects on attainment of a GED or high school diploma (where impacts were found on all but those who read below the sixth-grade level and those not receiving AFDC at baseline) and on use of a child care center or preschool fell into this category. With regard to pregnancy impacts, results were mixed: Sometimes they were larger for the more disadvantaged subgroup, sometimes for the less disadvantaged subgroup. There is some evidence that New Chance increased length of stay in education activities and increased scores on the Emotional Support subscale of the HOME scale more for the more disadvantaged subgroups; experimentals with more barriers to employment also experienced smaller earnings losses vis-à-vis controls than did experimentals with better workplace prospects. In general, however, the subgroup findings did not point to a consistent pattern of effects with regard to level of disadvantage. Finally, when differential subgroup effects were detected, they generally did not hold up across the outcome areas. For example, the program's impact on duration of attendance in education activities was especially large among Hispanic women, but the program's impact on GED receipt (as on most other outcomes) was similar for white, black, and Hispanic women.

In general, no site stood out as much better than the others across a wide range of impacts. Several factors make it difficult to detect or interpret cross-site impact findings. These include: the small sample size at many sites; differences in the measured and unmeasured characteristics of the populations served by the different programs; and different levels of service receipt by controls.[3]

III. Changing Lives, Uncertain Directions

A. Between Adolescence and Womanhood

Several studies suggest that the long-term outcomes of adolescent pregnancy may be more positive than the short-term outcomes (Furstenberg, Brooks-Gunn, and Morgan, 1987; Horwitz et al., 1991). For example, the evaluation of Project Redirection, a program for pregnant and parenting teens on welfare who were aged 17 and under, found impacts at five years after program entry but not at two years (Polit, Quint, and Riccio, 1988). One explanation for such improvement is that young mothers gain both increased maturity and greater stability as they make the transition from adolescence to womanhood. Adolescence unfolds differently for young people of different genders, ethnicities, and classes, and in different places, but generally is a period and a process during which young people try on and discard various roles and pursue various interests as they strive to define their identities, values, and goals.

Data from the New Chance monograph (Quint and Musick, 1994) as well as the findings presented in this report suggest that age-typical shifts of direction and periods of progress followed by periods of retrogression are common for the New Chance population. As one New Chance site director commented, "Participants don't necessarily move from Point A to Point B to self-actualization." Rather, their lives are characterized by flux and change – both the change common to adolescents generally and the special vulnerabilities and instabilities created by poverty. This change often makes for movement into and out of school, programs, jobs, and relationships. One striking finding of the monograph is that many of the young women interviewed for that study would have been portrayed quite differently if the interview had taken place a few months earlier – or, it is likely, a few months later.

---

[3] The smallest site enrolled only 69 sample members (experimentals and controls combined) and the largest, 171. Small sample sizes reduce the likelihood that even fairly large percentage differences will be statistically significant and, therefore, that the results can be ascribed to anything except chance.

Some sites may appear to have achieved larger impacts than others, but only because they enrolled a higher-than-average proportion of young women with characteristics found to be associated with larger impacts. When these between-site differences in sample members' measured characteristics were statistically controlled for, impacts, whether positive or negative, sometimes disappeared. Thus, e.g., Denver, Lexington, and Portland had significant effects on GED receipt before these statistical adjustments were made; after the characteristics of sample members at these sites were adjusted to be more equivalent to those of sample members elsewhere, however, the differences no longer were significant.

Finally, the behavior of controls, no less than that of experimentals, influences program impacts. That behavior was found to differ considerably from one site to another for most of the outcomes examined.

Because their lives are in flux, and because they are still growing up, young women who do not now appear headed in a positive direction may well alter their course in the future. What, then, can the results of the survey being administered at 42 months after random assignment – two years later than the early impacts presented here – be expected to show? There are two approaches to answering this question. First, over time, will young women in the experimental group and their children make more progress than control group members and their children, so that program effects will appear more positive over the long term than over the short one? Second, irrespective of research status, what are the life prospects of the young mothers in this study?

B. Program Impacts over Time

At this early stage, the findings provide grounds for both optimism and pessimism about the longer-term impacts of New Chance. At the time of the 18-month interview itself, the young women in the experimental and control groups resembled each other in many ways, with equal percentages of both groups in school or training, employed, or on welfare. Nonetheless, significant differences between the groups at the 18-month point – the higher rate of GED receipt among experimentals and their different living arrangements and, concomitantly, different fertility-related behavior – might make for different outcomes two years later. Furthermore, impacts on maternal behaviors and attitudes affecting children might also result in improved developmental outcomes for the children of experimentals.

1. Will a GED prove of value? Conceivably, there are two ways in which having a GED may improve labor market opportunities. First, it may have a direct value if it enables GED-holders who lack other credentials to get better jobs than they would have as high school dropouts. Whether a GED is indeed valuable in the labor market is a matter of considerable controversy, as Chapter 5 made clear. The long-term New Chance findings will help shed light on this issue. Indeed, it may be possible to determine the worth of a GED only over time if employers reserve their higher-quality jobs for those in their mid-20s. This hypothesis is roughly consistent with a national study of high school graduates that found that employers reward increased cognitive skills among 20-year-olds only a little or not at all, but are substantially more likely to hire 21-year-olds with higher skills, and to pay them more (Murnane, Willett, and Levy, n.d.).

The GED may also have an indirect value because of its "gatekeeper" function: colleges and training programs often admit only applicants who have already received a high school diploma or GED. While it is encouraging that a higher proportion of experimentals than controls were in college at follow-up, the higher rate of GED attainment among experimentals does not yet appear to have paid off in terms of higher rates of completion of skills training or entry into better-paying jobs, for several reasons. First, just over a third of the experimentals who earned a GED did not enter skills training or a work internship. Program staff reported that some wanted a "vacation" before making another investment of time, and that others did not want to work until their children were of preschool age. Furthermore, the data suggest that some young women who began skills training or college left before completing their courses of study. And although the employment findings in Chapter 8 were not reported separately for GED attainers and non-attainers, the case studies in the New Chance monograph indicate that many young women who got a GED and subsequently found employment did not keep these jobs.

Nonetheless, the experimentals' higher rate of GED attainment may place them at an advantage over controls if employers really do value GED attainment, especially among workers who are somewhat older than these young women were. It may also be worthwhile if GED-holders among the experimentals who did not subsequently enter skills training (or who entered training but then dropped out) decide to enroll in training or college at a later point, if they complete these activities, and if the combination of a GED and additional training and credentials results in better long-term employment prospects.[4] The many "ifs" in the preceding statement suggest that it is possible, but far from certain, that the GED will have a longer-term payoff.

2. **Will repeat pregnancies in the context of cohabiting relationships impede self-sufficiency?** Young women in the experimental group were more likely both to be living with a partner or husband at follow-up and to have experienced a post-baseline pregnancy than were their control group counterparts. Although a subsequent pregnancy, if followed by a birth, is likely to delay a young mother's progress toward self-sufficiency, living with a partner may also have positive implications for the well-being of the young women and their children, as the data in Table 9.2 suggest. However, the nonexperimental nature of the analysis, which compares three groups of experimentals (rather than experimentals and controls), means that the findings must be treated with caution.

The first three columns of the table present results on a variety of outcomes for experimentals in three different living arrangements – living with a parent or grandparent, with a partner or husband, or with their child(ren) only – at the 18-month follow-up.[5] To reduce selection bias (i.e., the fact that young women who "chose" different living arrangements were likely to have differed in other ways as well), the outcomes for the three groups were statistically adjusted to account for group differences in measured baseline characteristics. Such adjustments, however, could not take into account such *unmeasured* characteristics as differences in maturity, personal attractiveness, or interpersonal skills that might also help to explain different outcomes. A further limitation is that, for all outcomes pertaining to the entire follow-up period rather than to the respondent's status at the 18-month point, the time sequence of living arrangement and outcome is unclear.[6] The last two columns

---

[4] Participation in GED preparation activities was also higher among experimentals than among controls during the follow-up period. It is possible, therefore, that should members of both groups re-enroll in education activities, it will take less time for experimentals to complete a GED in the future.

[5] Together, these three groups included 87 percent of all experimentals. The remaining 13 percent lived with a variety of other people, including in-laws, siblings, aunts and uncles, cousins, other relatives, male and female friends, and non-relatives, such as boarders. The three groups were designed to be mutually exclusive. Thus, young women who were living with both parent and a partner were classified as "living with parent or grandparent." (The "parent" category included stepparents and foster parents.) Just over two-thirds of the young women who were living with a man in a conjugal relationship were not legally married.

[6] Thus, young women may have become pregnant and then started living with the baby's father, or the other way around; they may have met their partners at work, or the presence of a live-in partner who could provide child care may have made it easier for them to go to work.

TABLE 9.2

OUTCOMES FOR NEW CHANCE EXPERIMENTALS, BY LIVING ARRANGEMENT AT 18 MONTHS

	Living Arrangement			Statistical Significance of Outcome Differences (a)	
Outcome	Living with Parent or Grandparent	Living with Partner or Husband	Living with Child(ren) Only	Parent vs. Partner or Husband	Partner or Husband vs. Child(ren) Only
Education–related outcomes					
Participated in any education activity, quarters 1–6 (%)	86.2	84.1	86.1	n.s.	n.s.
In high school, a GED program, or college at 18 months (%)	18.9	10.7	17.3	***	**
Attended skills training, quarters 1–6 (%)	34.5	31.8	30.9	n.s.	n.s.
In skills training at 18 months (%)	7.4	4.9	7.2	n.s.	n.s.
Ever received high school diploma or GED by month 18 (%)	41.5	44.4	45.6	n.s.	n.s.
Fertility–related outcomes					
Ever pregnant during months 1–18 (%)	52.5	65.9	55.2	***	***
Had a planned pregnancy during months 1–18 (%)	5.9	13.3	6.2	***	***
Had an unplanned pregnancy during months 1–18 (%)	47.1	54.5	48.4	*	n.s.
Had an abortion during months 1–18 (%)	15.1	11.9	15.8	n.s.	n.s.
Gave birth during months 1–18 (%)	24.2	37.2	28.8	***	**
Not sexually active at follow–up (%)	25.4	4.9	18.9	***	***
Had sex without using contraception at follow–up (%)	26.9	34.9	28.5	**	*
Pregnant at 18 months (%)	13.5	19.1	12.5	*	**

(continued)

TABLE 9.2 (continued)

	Living Arrangement			Statistical Significance of Outcome Differences (a)	
Outcome	Living with Parent or Grandparent	Living with Partner or Husband	Living with Child(ren) Only	Parent vs. Partner or Husband	Partner or Husband vs. Child(ren) Only
Child-related outcomes					
HOME scale total score at follow-up (b)	100.7	102.3	100.4	n.s.	*
Emotional Support subscale score of HOME at follow-up	99.9	101.5	101.2	n.s.	n.s.
Child did not have father figure or see father at follow-up (%)	14.8	1.0	13.3	***	***
Employment- and welfare-related outcomes					
Employed, quarters 1-6 (%)	44.4	45.4	38.1	n.s.	**
Average total earnings, quarters 1-6 ($)	1,350	1,798	1,168	*	***
Employed at 18 months (%)	21.5	23.7	17.0	n.s.	**
Received AFDC at 18 months (%)	86.0	77.8	92.9	***	***
Other outcomes					
In school, training, or work at 18 months (%)	38.2	34.4	33.3	n.s.	n.s.
Average CES-D depression score at follow-up (c) (%)	16.8	15.3	15.9	*	n.s.
Sample size	401	324	502		

SOURCE: MDRC calculations from survey data.

NOTES: Calculations for this table used data for 1,227 experimentals for whom there were 18 months of follow-up survey data and who were living in the specified arrangements at 18 months.

(a) A two-tailed t-test was applied to the difference between average outcomes for the two groups compared in each set of comparisons shown in this table. Statistical significance levels are indicated as *** = 1 percent; ** = 5 percent; * = 10 percent; "n.s." indicates that the difference was not statistically significant at the 10 percent level.

(b) A modified version of the short form of the Home Observation for Measurement of the Environment (HOME) Scale (first administered in the National Longitudinal Survey of Youth) was administered. Scores here were age-standardized to have a mean of 100 and a standard deviation of 15.

(c) Those with scores below 16 on the CES-D are considered not to be at risk of depression; those with scores of 16 and above are considered at risk.

present the results of tests of the statistical significance of the differences in outcomes for those living with a parent and those living with a partner, and the difference between those living with a partner and those living with their children only.

The table indicates that experimentals living with a partner at follow-up indeed had very different patterns of fertility-related behavior than those in either of the other living arrangements: higher rates of pregnancy, both planned and unplanned; a higher rate of births; a greater likelihood of being pregnant at the time of the 18-month interview; and a greater likelihood of engaging in unprotected sex. They were also less likely to be attending an education program or college at the 18-month point.

Nonetheless, if getting pregnant was a risk of cohabitation from the standpoint of self-sufficiency, there were many countervailing benefits in the areas of both economic and emotional well-being, especially when compared with living on one's own. Those living with a partner or husband were significantly more likely than those living with only their children to have been employed during the 18 months and also at the time of the interview; they had much higher earnings and were considerably less likely to be receiving AFDC than were young women in either of the other two groups. Predictably, their children were significantly more likely to have access to a father or father figure. Those living with a partner or husband also registered lower depression scores than those living with a parent. Along with these statistically significant and positive outcomes associated with living with a partner or husband, it is also worth noting that equal proportions of young women in all three living arrangements received a GED or high school diploma, and equal percentages, too, were engaged in a skill-building activity (school, training, or work) at follow-up.

What is not clear is the stability of these cohabiting relationships. If repeat pregnancy takes place in the context of a stable relationship, then young women may not experience particularly adverse consequences for economic well-being if they have their children relatively close together. On the other hand, if these relationships end, the young women may lose some of the advantages associated with living with a partner or husband and will have one or more additional children to provide for.[7] Over time, it will become evident whether the fertility-related behavior of New Chance women is taking place in a context marked by greater or less interpersonal stability than that of controls.

3. Will the children of experimentals exhibit better developmental outcomes than those of controls? As a group, children of young mothers who are poor are at high risk of adverse cognitive and behavioral development. Several results suggest that the children of experimentals may benefit from their mothers' participation in New Chance, although this is far from assured. For one thing, the program increased the children's exposure to center-based care that was generally of good quality, and it did so at an important early point in their development. Whether the relatively brief period for which they were enrolled in such care will make a long-term difference is an open question.

Experimentals registered modestly (although significantly) more emotional support for their children and less authoritarian attitudes, although the program had no impact on a summary measure of the home environment in which children of experimentals and controls were being raised (in part because this measure is quite sensitive to a household's economic well-being, an outcome that was unaffected at this early stage). If New Chance produces positive changes in the young mothers' economic status over time, then such changes, together with these differences in experimentals' parenting practices, may make for more favorable developmental outcomes in the next generation.

---

[7] More effective paternity establishment and child support enforcement procedures could mitigate some of the economic consequences of instability in these relationships.

The 42-month follow-up survey includes a direct assessment of the cognitive development of the children of both experimentals and controls, as well as measures of their social and emotional development and school performance. These findings will be incorporated into the final (1996) report on the impacts of New Chance. They will provide information of interest to policymakers, program planners and operators, and others concerned with the effects of interventions designed to benefit both parents and their children.

4. **Will experimentals' employment and earnings outstrip those of controls, and will their welfare receipt be reduced?** Enrollment in New Chance entailed an investment in education and training, rather than direct entry into the labor market. Consequently, experimentals registered lower employment and earnings than did controls, but the difference was significant only during the first part of the follow-up period; during the last two quarters of follow-up, equal percentages of both groups were working. Despite controls' higher employment rates, AFDC receipt for both groups was substantially similar throughout the 18-month period.

Given the relatively brief period of forgone employment experienced by experimentals and the converging employment rates of the two groups, it is possible that experimentals' rates of job-holding will, over time, surpass those of controls, especially if the experimentals accrue more education. At this juncture, there is little clear evidence either bolstering or refuting this possibility.

C. How Far to Self-Sufficiency?

A focus on outcomes for experimentals vis-à-vis controls may be an overly narrow perspective from which to view the future life trajectories of the young women in the study. The data suggest that neither group of young mothers could be considered to be doing well. Rather, the majority of young women in both groups appeared to be a considerable distance from self-sufficiency.

Thus, over half of the young mothers lacked either an educational or vocational credential. The majority had no employment experience of any significant duration. Those who worked held jobs that paid, on average, less than $5 an hour, usually with no health insurance; if they worked for 40 hours a week, 52 weeks a year at this level, their earnings would fall short of the poverty line for a family of three. Over half of the young women in both groups became pregnant during the 18-month follow-up period, and the majority of these pregnancies went to term, or were slated to do so. At the 18-month point, 82 percent of the women in both groups were receiving public assistance.

It is these absolute levels of disadvantage that those concerned with reforming welfare will need to take into account as they design programs and policies. The next section deals with the nexus between welfare reform and the realities of the young mothers' lives.

IV. New Chance and Public Policy

The New Chance results need to be considered in light not only of the young women's changing lives but also of the policy context in which their behavior has unfolded. That policy context has undergone major changes since the inception of the demonstration and continues to evolve in new directions.

Welfare reform proposals now under discussion pick up the theme of mandates and obligations sounded in the Family Support Act of 1988 and seek to strengthen these requirements. Proposals currently receiving the most attention are predicated upon a two-year limit on receipt of welfare benefits without work. Within those two years, recipients would be encouraged and required to participate in education, training, or other activities to increase their employment. If they had not secured a job by the time the two years expire, they would have to work or lose aid (or a portion of it). Some proposals would also require unmarried minor mothers to live with a parent in order to collect welfare.

For the most part, New Chance was implemented as a voluntary program; thus the New Chance data cannot directly speak to the merits or drawbacks of these welfare reform initiatives. Nonetheless, the findings may serve to illuminate fundamental issues in the design of these initiatives, especially as they relate to young mothers. Several such issues are discussed below.

However, it is important to recognize that if the welfare-to-work proposals now under discussion are implemented on a massive scale, they have the potential to change the very nature of welfare. It is possible that they will radically alter community-wide beliefs, attitudes, and expectations, as well as the behavior of welfare recipients and prospective recipients. Further, if measures to "make work pay," such as advance payment of an expanded Earned Income Tax Credit, increased subsidized child care for the working poor, and universal health insurance, are put in place, the incentives for working versus remaining on welfare will shift dramatically. In these events, neither New Chance nor any other current program, mandatory or voluntary, can provide adequate guidance about how young mothers (or anyone else) will respond.

1. **What level of participation could be expected, and for how long?** The high participation levels evident in New Chance are predictable in voluntary programs that attract a relatively motivated group of participants. In fact, 94 percent of young women in the New Chance experimental group – and 82 percent of the controls as well – were active in education, job training, work experience, or paid employment during the 18 months of follow-up.

The Teenage Parent Demonstration was a mandatory program that served all first-time teen mothers on welfare (including in-school students and high school graduates as well as dropouts and, presumably, less as well as more motivated teens) and used sanctions (i.e., reduction of the welfare grant for those who did not comply with program rules) to achieve its goals. That program also achieved substantial participation: 68 percent of the young mothers in that demonstration who were high school dropouts participated in school, job training, or employment within 24 months after program entry. Importantly, over half of the controls studied (53 percent), who were under no obligation to do anything and were not penalized for their inaction, also took part in one of these activities.

These data suggest that many young welfare mothers, despite their responsibilities for the care of very young children, actively take steps to change and improve their lives. From a policy perspective, the problem may be less their lack of effort than the lack of consistency and continuity in that effort and its frequent interruptions. The New Chance data on absenteeism, early termination, dropping out of college and training, and job turnover all speak to this issue and suggest its pervasiveness in many spheres of activity. Pregnancy, of course, is often a concomitant of interrupted progress (if not necessarily its cause).

Would participation requirements and time limits add urgency to young women's efforts, increasing their commitment and persistence in building the skills needed for better-paying jobs? Would such circumstances induce them to make the transition from adolescence to responsible adulthood more quickly than they would otherwise? These questions cannot be answered directly, and New Chance coordinators, asked about the wisdom and likely effects of making the program mandatory, offered mixed responses.[8] In general, it seems reasonable to conclude that a mandate might well increase participation. But it is not clear that increased participation would lead to increased self-sufficiency. Some young mothers may be positively affected by a participation requirement, while others (along with their children) may be harmed by repeated sanctions for noncompliance.

The New Chance data suggest that a model that envisions an "investment period" of 24 *consecutive* months would be inappropriate for this population, since many young women would not use this period well: Although they might benefit from two years of education and training, they would be unlikely to participate continuously in these activities. (On the other hand, young mothers who persevere in a two-year community college program may well need more than two years to complete it, because they may need to take non-credit remedial courses or to drop some classes in which they are enrolled.) Program planners will need to decide whether welfare recipients are "entitled" to receive a full 24 months of education and training and to determine the appropriate course for those who do not want to, or cannot, make a continuous investment. For example, if a young woman completes a GED within five months, goes to work and off welfare for seven months, and then decides to enter a training program, will she have 19 months of welfare-supported training available to her? The New Chance results suggest that interruptions of school and training are commonplace, not exceptional, so that program administrators can expect to confront decisions of this type routinely.

2. **What circumstances in the young mothers' lives would cause the time limit "clock" to stop ticking and start up again?** The New Chance findings suggest that a young woman's ability to participate continuously in education and training is likely to fluctuate over time, and with changing

---

[8] Staff at six sites favored a participation requirement, believing that it would improve attendance (or had done so among those young women who were co-enrolled in JOBS as mandatory participants), and that it would make the program more like the "real world," in which absenteeism and poor performance are penalized. At two sites, program coordinators were strongly opposed to mandatory participation: The coordinator at one program, located in an area of high-density urban poverty, commented that participants had "too many insurmountable problems" that could interfere with participation, while the second coordinator spoke of the resentment created by such a requirement, expressing the view that real change can come only when people are ready and cannot be forced. At the remaining seven sites, coordinators believed that a mandatory requirement would not have affected participation at all, or had mixed views. (At one site, the coordinator was new to the program, and her opinion was not solicited.)

circumstances. Some of these are circumstances she may have helped to bring about, others are out of her control. In New Chance, absenteeism was due in part to lack of motivation, apathy, lack of habituation to a routine, and disaffection from the program. It also resulted from homelessness (program staff reported that almost half of the young women with whose situations they were familiar did not have a stable place to live at some point); substance abuse on the part of enrollees or their family members or partners; domestic violence; child care and transportation problems; the young women's own illnesses and those of their children; conflicting welfare and medical appointments; and discouragement from significant others.

Pregnancy is one of the most common events that interrupt participation and might stop the clock. Given the results of New Chance and of other demonstrations serving young mothers, it is far from clear whether and how their pattern of closely spaced childbearing can be altered. If subsequent pregnancies and births continue to be frequent occurrences after welfare reform measures are enacted, welfare planners will need to formulate policies concerning whether and for how long during pregnancy young women will be expected to participate in education or training, and at what point after giving birth they will be expected to resume.

Policymakers will need to consider these and other circumstances under which young women interrupt or cease their involvement in education and training activities, and procedures will need to be developed for insuring that, once a period of deferral is over, the clock is reactivated.

3. **What would happen to those who lose their private-sector jobs**? It is notable that over 40 percent of both experimentals and controls were employed at some point during the 18-month follow-up period. But job-holding was, in the majority of instances, accompanied by rapid job loss. About half the jobs that began during the first year after random assignment lasted three months or less. The majority of jobs ended when sample members quit them, reporting reasons that included problems with supervisors, the lack of good child care, insufficient pay and inconvenient hours, and pregnancy.

It may be possible to design interventions that can better prepare young people for the demands of the workplace (demands that are interpersonal as well as skills-related), that can help them negotiate the difficult first months of employment with fewer mishaps along the way, or that can help them find new jobs rapidly if their initial experience proves unsatisfactory. The value of developing and evaluating such interventions is discussed in the next section of this chapter.

It is also possible that, in the new environment envisioned by welfare reform proposals, job turnover will be reduced, since welfare will be less attractive and work may be more so. If quick job loss remains the norm, however, policymakers will need to grapple with an array of associated issues: whether temporary assistance will be available to those fired from their jobs until they can find employment again; how to deal with those who quit their jobs – sometimes for good reasons, sometimes for reasons that reflect their immaturity and lack of judgment; what will happen to those who accept jobs that are seasonal or otherwise temporary when these jobs end.

4. **How would a community service employment program be structured**? In some proposals, a community service employment program will be created for individuals who are unable to find private-sector jobs. In some plans, individuals would be expected to "work off" their welfare grants – implying work for a fixed number of hours per month, the specific scheduling of which

might be flexible. In other plans, they would work for wages (presumably set at the minimum-wage level) and with a fixed work schedule, and would presumably be docked for poor attendance. In a pay-for-performance scheme, it is unclear whether young women whose New Chance attendance was sporadic for all the reasons noted above would be better able to keep to a firm work schedule, or whether they would be thrust even more deeply into poverty.

5. **How would programs be staffed?** The difficult psychological and interpersonal problems faced by many young mothers – problems that frequently interfere with progress toward self-sufficiency – suggest that staff members of welfare-to-work programs need counseling expertise and experience. Regular in-service training to help staff members deal with especially difficult issues is also indicated.

The findings on job turnover in this report and in the New Chance monograph indicate that many young women need thoughtful, interpersonally skilled supervisors if they are to succeed in the workplace and learn workplace norms and behavior. Program planners may want to consider how a community service employment program can be supportive without undermining participants' incentive to seek private-sector employment.

6. **What level of work effort would be considered acceptable in both private-sector and community service jobs?** About one-third of New Chance sample members who worked did so for 35 hours a week or more; 45 percent worked 25 hours or fewer. Some would contend that part-time work is a reasonable compromise between society's expectation that young mothers not be permitted to remain idle at home and its desire for mothers to be available to support the development of their children, especially when the latter are very young and when they live in dangerous environments. But it is not clear whether there are enough part-time jobs available in the private sector, whether the young mothers in the New Chance target group would be able to get these jobs, and whether the jobs would enable young women to leave public assistance. Policymakers will need to decide whether part-time employment constitutes grounds for exemption from the time limit, allowing mothers to receive supplemental benefits for themselves and their families, as well as whether mothers would be allowed to go to school part-time and work part-time while still receiving assistance.

7. **What level of child care use could be expected?** In New Chance, high levels of participation in school, training, or work were associated with high levels of child care utilization. Thus, 95 percent of the experimentals and 85 percent of the controls used some form of non-maternal care during the 18 months. Use of "market care" (i.e., a day care center, preschool, or paid babysitter or family day care home) was consistently higher in the experimental than in the control group. The New Chance sites offered on-site care and otherwise encouraged use of day care centers because of their reliability and potential for stimulating cognitive development; thus, New Chance is not an ideal test of how much young mothers would depend on such care in order to participate in welfare-to-work programs or employment.[9]

---

[9] A study of child care utilization by participants in California's GAIN program, a welfare-to-work initiative, found that 10 percent of all mandatory registrants (those with children aged 6 years and over) and 39 percent of all volunteers (those with children aged 5 years old and under) used GAIN-subsidized child care (Martinson and Riccio, 1989). This care could be supplied by government-licensed day care homes and

(continued...)

The data suggest, however, that it would be expensive to provide market care, especially given the high cost of infant care and the large proportion of young mothers with children under age 1. The on-site care provided by the New Chance programs ranged in cost between $236 and $1,006, and averaged $420, per slot per month. These expenditures purchased care that was of good quality – an important consideration for all children, but especially for disadvantaged children, for whom high-quality care has been found to have positive effects on development.[10]

8. **What policies to "make work pay" deserve special consideration?** Program operators reported that the funding structure for child care is one of the most formidable barriers to continued employment and progress toward self-sufficiency. Those enrollees who obtained jobs generally earned modest wages and were not in a position to develop a cash reserve of savings early on. Yet, child care centers generally required that the young women prepay the cost of child care at the beginning of each month – at the same time rent was due – and welfare reimbursements of these expenses were often delayed in coming. Furthermore, transitional child care under JOBS is not available for those who are employed but still receiving AFDC (as is often the case during the first months of employment, while income disregards are still in effect) or for those working less than full-time.

Altering these child-care-related provisions would be a significant advance toward "making work pay" for young mothers.

9. **If a requirement that young mothers live with a parent or another adult were imposed, what effects could be expected?** Such requirements are intended to remove any incentive for a teenager to have a child in order to establish her own household supported by welfare. They also reflect the belief that the children of young mothers would be better off being raised in households where adults are present, and that parents would be in a better position to encourage and monitor their daughters' school attendance.

This study obviously does not address the issue of preventing first pregnancies and births, since all program participants had one or more children at the outset. Table 9.3, however, sheds light on the question of the extent to which behavior might be different if young mothers were required to live with their parents or other adults. The table presents impacts across a variety of areas for two subgroups of sample members, defined by whether or not they lived with their parent(s) or other adults (other than spouses or partners) at baseline. Because the table compares experimentals and controls, and because subgroups were designated according to their status at random assignment, these results (unlike those presented in Table 9.2) are fully experimental, and their validity is not threatened by selection bias.

---

[9](...continued)
centers as well as by providers who were not required to be licensed (such as an enrollee's family and friends). Most of those who did not use this assistance did not meet the criteria for receiving it: having a child under the age of 12, attending a program orientation, and participating in subsequent program activities.

[10] Quint and Musick (1994) suggest that high-quality child care may be critically important to compensate for the inability of some young mothers to provide their children with a home setting that makes for adequate, let alone optimal, cognitive, linguistic, and socioemotional development.

TABLE 9.3

IMPACTS OF NEW CHANCE AT OR WITHIN 18 MONTHS AFTER RANDOM ASSIGNMENT FOR SAMPLE MEMBERS LIVING AND NOT LIVING WITH THEIR MOTHERS OR OTHER ADULTS AT RANDOM ASSIGNMENT

Outcome and Living Arrangement at Random Assignment	Experimentals	Controls	Within-Subgroup Impact (a)	Significance of Between-Subgroups Impact Difference (a)
Average number of weeks in education activity, quarters 1-6				n.s.
Living with mother or other adult (b)	25.3	13.9	11.4 ***	
Not living with mother or other adult	26.1	14.0	12.1 ***	
Number of weeks in skills training, quarters 1-6				n.s.
Living with mother or other adult (b)	7.7	4.9	2.7 ***	
Not living with mother or other adult	7.4	5.4	2.0 *	
Ever received a high school diploma or GED (%)				n.s.
Living with mother or other adult (b)	41.5	28.4	13.1 ***	
Not living with mother or other adult	45.2	33.1	12.1 ***	
Had a subsequent pregnancy in months 1-18 (%)				n.s.
Living with mother or other adult (b)	55.6	51.8	3.7	
Not living with mother or other adult	59.2	53.2	6.0	
Score on Emotional Support subscale of HOME (c)				n.s.
Living with mother or other adult (b)	100.5	98.9	1.6 *	
Not living with mother or other adult	100.7	100.3	0.5	
Used regular child care during months 1-18 (d) (%)				n.s.
Living with mother or other adult (b)	94.8	83.8	11.1 ***	
Not living with mother or other adult	95.9	86.9	9.0 ***	
Average earnings, quarters 1-6 ($)				n.s.
Living with mother or other adult (b)	1,289	1,608	-319 *	
Not living with mother or other adult	1,500	1,913	-413 *	
Sample size	1,408	680		

SOURCES: MDRC calculations from New Chance Enrollment Form and survey data.

NOTES: Calculations for this table used data for all 2,088 sample members for whom there were 18 months of follow-up survey data, including those with values of zero for outcomes and New Chance enrollees (i.e., experimentals) who did not participate in the program. The reported sample sizes may fall short of this number because of missing or unusable items from some sample members' questionnaires.

The averages or percentages are adjusted using a two-way analysis of covariance procedure controlling for up to 51 kinds of difference in characteristics, other than the characteristic used to define subgroups, before random assignment. The two categories used as factors were research status (i.e., membership in the experimental or control group) and, one at a time, the baseline characteristics indicated. Rounding may cause slight discrepancies in sums and differences.

(a) A two-tailed t-test was applied to each regression-adjusted within-subgroup impact and also to each difference between subgroup impacts. Statistical significance levels are indicated as *** = 1 percent; ** = 5 percent; * = 10 percent; "n.s." indicates that the difference was not statistically significant at the 10 percent level.

(b) This category includes those living with mother, father, stepparent, other adult relatives, and adult non-relatives.

(c) A modified version of the short form of the Home Observation for Measurement of the Environment (HOME) Scale (first administered in the National Longitudinal Survey of Youth) was administered. Scores here were age-standardized to have a mean of 100 and a standard deviation of 15.

(d) Regular child care was defined as an ongoing arrangement used while the mother was in school, in training, or working.

The data do not point to a clear conclusion about whether requiring older teens or young adults to live with their own mothers or other adults would ensure greater progress toward self-sufficiency. The first thing to be noted is the last column, which indicates that in no case was the difference between the impacts for the subgroups large enough to be statistically significant. In other words, program effects for those living with their mothers or other adults and those living in other arrangements at random assignment were substantially similar. It is noteworthy that, in absolute terms, both experimentals and controls who were not living with their mothers had somewhat higher rates of subsequent pregnancy, a potential impediment to self-sufficiency; on the other hand, they also had higher earnings, indicative of better prospects for economic independence.[11]

V. Implications for Program Design

Whether or not the long-term impacts of New Chance turn out to be more positive than the short-term results, and whether New Chance and similar programs are implemented in voluntary or mandatory contexts, the findings to date indicate areas in which the program fell short and suggest directions for improvement. In a more speculative vein, they also raise fundamental questions about content and structure that designers of programs for disadvantaged adolescents and young adults may want to consider.

A. Improving Program Practice

The evaluation results suggest the need to address systematic weaknesses in several areas of program implementation. Although MDRC anticipated problems in these areas and provided initial technical assistance to sites to improve program practice, the findings of the process analysis indicate persistent implementation issues. Both these findings and the strategies that local program staff have themselves developed to strengthen program components point to changes that seem likely to improve short-term outcomes and might also result in improved impacts. That others have made similar recommendations underscores the fact that these areas are both especially important and particularly difficult to implement well.

---

[11] It is worth noting that, among experimentals, black young women were slightly more likely to be living with a parent or a grandparent at follow-up (30 percent) than were either Hispanics (28 percent) or whites or others (26 percent). However, they were considerably more likely to be living with only their children (40 percent of blacks, 34 percent of Hispanics, and 29 percent of whites and others were in this arrangement) and less likely to be living with a partner (16 percent of black young women, 25 percent of Hispanics, and 34 percent of whites). New Chance had a statistically significant effect on living arrangements among blacks: 33 percent of black controls, but only 25 percent of their experimental counterparts, were living in their mothers' households at follow-up.

Of relevance to this discussion is the research of Testa (1992), who describes the results of a panel study conducted in the early 1980s of AFDC recipients under 18 years old living in Chicago and suburban Cook County, Illinois. That study found that black teens remained in their mothers' households longer (and on welfare longer, as well) than young women in other ethnic groups. Living in parental households enabled them to remain in school and complete their educations; moving away from the family of origin, along with marriage, were both associated with a higher probability of educational disruption.

The finding that New Chance experimentals were more likely to live apart from their mothers as a result of program participation might therefore be a cause for concern. However, there were strong GED impacts for blacks, as for members of the other two groups.

1. **Enunciating and enforcing attendance standards.** It seems likely that New Chance would have had stronger effects if young women had attended more regularly. Programs invite greater absenteeism if they fail to articulate clear attendance standards, reward good attendance, follow up quickly on absentees, and penalize consistently poor attenders (ultimately by dismissal from the program, if necessary). The New Chance sites that did all these things did not necessarily have good attendance. But those that *did* have good attendance also had clear rules and expected students to adhere to them.

2. **Improving family planning services.** The implementation findings indicate at least some of the reasons why New Chance was not more successful in helping participants defer subsequent pregnancies and point to areas where improvements might be sought.

First, enrollees need to have a clear, consistent, and regularly repeated message about pregnancy postponement. In programs like New Chance, young women's maternal roles are recognized – and celebrated – and it is easy for enrollees to hear mixed messages about the importance of deferring pregnancy. These messages can be made more consistent if the emphasis is on creating a better future for the children young mothers already have by deferring subsequent childbearing until a later point.

Second, some otherwise competent case managers, because of lack of time or because they did not necessarily feel comfortable or sufficiently knowledgeable in talking about sexuality, tended to bypass this issue in meetings with participants. This suggests the potential value of continuous in-service (as well as initial) training about sexuality and contraception, to help ensure that staff are well-informed and to afford them opportunities to share problems and solutions. If, despite such training, a case manager remains ill at ease, then responsibility for counseling participants about family planning and monitoring their contraceptive practice should be shifted to another staff member who is more comfortable in this area.

Third, staff may need to be especially attentive to participants' contraceptive practices at times of change and transition, or of special stress: when they have a new partner, or have begun living with a man, or are experiencing difficulty in the program, or are ready to move on to a new stage of program activities. It appears that these may be times of reduced contraceptive vigilance and of heightened vulnerability to a new pregnancy.

Finally, the data in Chapter 6 suggest that, while women in the experimental group were more likely than controls to report that they expected to have another child in two to four years, over 70 percent of the young women in both groups said either that they expected to have no more children, or that they expected to have their next child in five or more years. Given this interest in long-term deferral of pregnancy, it makes sense for program staff to encourage the use of longer-acting contraceptives such as Norplant and Depo-Provera – methods that, unlike the condom, are entirely within the woman's control.

Yet, it bears repeating that altering fertility-related behavior has proved a formidable challenge that no program for young mothers has yet mastered. The difficulty of inducing young women to practice contraception consistently once they have become mothers points to the need for more, and more effective, efforts aimed at forestalling first births to teenagers.[12]

3. **Strengthening employment-related components.** At most sites, employment-related activities, with which sites had had relatively little experience, were the weakest part of the New Chance treatment. Instructors often had other responsibilities and only limited time to develop interesting classes, and few curricular materials were available to make their task easier. Yet, because of their relative novelty, these activities may need even more attention and more careful planning than other program components. The employment-related goals of the program need to be kept in focus – and the enrollee must be kept focused on them from the beginning of her program stay.

The rapid job turnover experienced by sample members suggests the need for employability development sessions to address not only job-seeking skills and techniques but also, and equally, job retention. Problems with supervisors or attitude problems accounted for a quarter of all instances of job-quitting. Through role-plays, presentations by outside speakers, and other techniques, students can learn about employers' expectations and workplace norms and practices.

Job development and placement efforts also need to be bolstered and expanded. New Chance job developers often worked only with the relatively small number of young women who completed a GED (or, indeed, the full sequence of program activities). Helping all enrollees to get better jobs than they would have otherwise is a critical way in which programs can assist young people.

4. **Bolstering the parenting component.** Over time, sites found ways to strengthen the parenting component and make it more appealing to enrollees. At one site, for example, several program enrollees were trained as peer facilitators of the parenting sessions; site staff reported that other participants responded well to this arrangement, preferring to learn about good parenting practice from their age peers rather than from older adults. Sites have also placed increased emphasis on inculcating parenting skills through sessions involving mother-child interaction rather than through direct instruction.

5. **Incorporating mental health and substance abuse services.** Several sites found it useful to incorporate additional mental health services into the program model to respond to the issues enrollees presented. Thus, some sites developed consulting agreements with community mental health centers or other providers whose staff members could provide skilled counseling to the young women. One site established an incest survivors' support group.

Substance abuse was another issue with which some sites found it useful to seek the assistance of outside agencies. Sometimes the enrollees themselves had significant substance abuse problems. (In fact, since the period under study in this report, one site instituted drug testing upon program entry and continued follow-up until the participant was "clean.") In other instances, enrollees needed assistance in dealing with the substance abuse problems of their boyfriends or family members.

---

[12] It is also worth noting that abortion played a major role in the fertility-related behavior of both experimentals and controls. If access to abortion were eliminated or restricted, higher rates of childbearing could be expected.

6. **Developing incentives and rewards for less able students.** Not everyone could be helped to attain a GED within a relatively short time period. The data suggest that not only were the poorest readers unaffected by New Chance in terms of GED attainment, but they also participated for fewer hours than other enrollees. It seems reasonable to hypothesize that their reduced participation in part reflects discouragement about the pace of their progress, especially when they could see their peers advancing more rapidly toward a certificate.

Programs face a challenge in keeping less able students motivated. To do so, they need to find ways of rewarding participants on the basis of effort, not just accomplishment, and of dividing major accomplishments into smaller, more achievable goals and rewarding attainment of each of these. These rewards may take the form of concrete incentives; they may also simply mean that staff give as much attention to, and express as much pleasure in, the achievements of those who are struggling as of those who are successful.

It also seems plausible that less academically able students will remain more motivated if they have other than academic goals toward which to strive – another reason for ensuring that all young women move into employment-related activities at a relatively early point.

7. **Reaching out to family members and partners.** New Chance enrollees do not exist as isolated individuals; they are enmeshed in complex social networks whose members influence their choices and behavior. Thus, contraceptive decision-making may be influenced by a boyfriend's desire for a child – or a mother's desire for a grandchild. Parenting behavior is also shaped by familial practices and role models.

Programs like New Chance need to reach out to these "significant others" – to neutralize potential opposition to program messages where possible and to enlist the support of these individuals in fostering the young women's progress toward self-sufficiency.

B. Re-examining the Program Model

At this point, it is too early to draw conclusions about the effectiveness of the New Chance model. Yet, given the high cost to society of the target population and the need for a continuing search for promising service strategies, it also seems important to note three key questions about program design that planners may want to consider, even in the absence of evidence about the effectiveness of alternative approaches.

1. **Should education and training services be offered concurrently or sequentially?** The sequential structure of education and training in New Chance that was adopted at all sites except Portland was intended to ensure adequate time and attention to the personal development and parenting components included in the program model.[13] But this structure has marked disadvantages as well.

---

[13] Since the period of program operations studied in this report, other New Chance sites have placed increased emphasis on training and have offered it concurrently with education.

If vocational training is deferred, participants – and staff as well – may lose sight of the program's long-term employment objectives and focus exclusively on the attainment of more immediate education goals. The records of both the JOBSTART Demonstration and New Chance suggest that fall-off in participation between the education and training phases in sequential programs may be virtually inevitable, especially when training takes place away from the initial setting.

Evidence on the impacts of alternative program structures is inconclusive. In JOBSTART, some programs of both types had impacts on enrollees' employment; some did not. It is worth noting, however, that the site that produced the largest earnings impacts in that demonstration, the Center for Employment Training (CET) in San Jose, was of the concurrent type. (CET was also notable for its integration of education and training and its intensive job development efforts, and these factors might also have contributed to the program's substantial impacts.) The JOBSTART research sample at CET did not include young mothers; however, a study of employment and training efforts directed toward minority female single parents also found the CET treatment to be especially effective (Gordon and Burghardt, 1990). One aspect of concurrent programs needs to be recognized, however: They must make training available at or near the program location. The number of sites able to offer training along with education and other services is considerably smaller than the number that can offer education and other services alone. Nonetheless, the weight of the findings suggests that it is worth testing concurrent programs more widely for a young-mother population.

2. **Should more emphasis be placed on following up program enrollees after they leave the program site for training or employment?** Young women who found jobs were frequently unable to hold them for very long; those who entered college or training programs often dropped out.

As noted earlier, although the New Chance guidelines stipulated that case managers remain in contact (first biweekly, then monthly) with participants after they moved into Phase II components or into employment, rising caseloads and the demands placed on their time by new program entrants often made it difficult for them to conduct regular, detailed follow-up efforts. But participants might well benefit from even more frequent counseling by case managers or other helping professionals as they move into new and unfamiliar terrains. Such staff members could assist them in working out issues with supervisors and other colleagues at the workplace, or in finding new jobs if necessary (or if a new position would offer more opportunities for advancement). They could also help the young women develop strategies to resolve problems (e.g., child care and family issues) that could interfere with progress, or refer them to other agencies providing such assistance.

Who should provide such assistance, what it should entail, how often contact should take place, and for how long are all open questions. The specific answers are likely to vary with the individual client: her current activity, her level of maturity, the areas in which she needs immediate assistance, her ongoing problems, the helping network she already has, and other factors. These decisions are also, of course, a function of budgets: Case management and counseling are expensive services and tend to be reduced when agencies face fiscal constraints.

Whether such assistance could help smooth the often twisting course young mothers take in pursuing self-sufficiency is uncertain. But it appears important to determine whether ongoing guidance and support will help young women sustain progress over time.

3. **Is a comprehensive treatment indicated for all participants?** New Chance was designed to address a set of problems that, experts agree, commonly affect young mothers. Yet, the findings raise an important question: Should programs provide all enrollees with a common set of

services and try to do everything at once, or should they concentrate on a few issues of primary importance?

It is obviously impossible to answer this question definitively. Comprehensive programs are thought to be necessary on the assumption that unaddressed problems in many areas of their lives can derail young mothers' progress in other spheres. The comprehensive approach adopted by New Chance "worked" in the sense that young women in the experimental group received more services than did the controls. However, on average, experimentals received only a limited amount of service in many of these areas, and this sparse treatment did not translate into impacts.

Were some New Chance participants unable to take in all these components at the same time? Did the program set too high a threshold for participation, leading some young women to be overwhelmed? In this regard, it is perhaps relevant that the program's most consistent impacts were on GED attainment – a goal that the young women themselves valued and for which they actively sought program assistance. In the areas of parenting or family planning, where the young women were less likely to define themselves as needing assistance, New Chance registered effects that were small, insignificant, or in the opposite direction of what had been expected.

This suggests that programs need to be equipped to address a wide range of concerns (on their own or through referral to other agencies), but that they may want to begin by helping participants tackle issues they see as paramount. Alternatively, it may make sense to make some services mandatory and others available when participants are ready to deal with them. It may be that the real issue is not what people need but what they can incorporate and use at any given time.

APPENDICES

APPENDIX A

A COMPARISON OF RESEARCH GROUPS

APPENDIX A

A COMPARISON OF RESEARCH GROUPS

This appendix contains an assessment of the effectiveness of the experimental design of the New Chance evaluation. It compares the baseline characteristics of experimentals with those of control group members and measures the statistical significance of apparent differences. Based on this comparison, it is concluded that there are no systematic or statistically significant differences between experimentals and controls, as intended by the research design.

As discussed in Chapter 2, random assignment was incorporated in the research design of the New Chance evaluation to create a valid counterfactual for the experiences of sample members who were exposed to New Chance. Random assignment intends to create experimental and control groups that are balanced on all baseline characteristics, measured and unmeasured, which may affect relevant outcome measures. As a result, any differences between the two groups that are found after random assignment can be attributed to the program.

A systematic comparison of baseline characteristics can be used to verify that random assignment indeed succeeded in creating two balanced research groups. Table A.1 compares selected baseline characteristics for experimentals and controls. Statistical tests were performed to evaluate the statistical significance of experimental-control differences on the various baseline measures. As expected, differences were generally small and most of them not statistically significant.

It is difficult to assess the overall effectiveness of random assignment from a broad set of bivariate comparisons such as featured in Table A.1, because the large number of t-tests is likely to generate differences that are "statistically significant" by chance.[1] Also, the baseline characteristics that underlie the statistical tests may not be entirely independent of one another. As a result, one significant difference in Table A.1 may generate another.

To address these problems, a multivariate analysis was used to measure the differences between research groups in one statistical procedure. This procedure tests the hypothesis that experimentals and controls are drawn from the same population by attempting to discriminate between the two groups using baseline characteristics. The actual test is a joint F-test for the significance of a set of coefficients in the following regression equation:

$$STATUS=\beta_0+\sum_x \beta_x X_x+\epsilon$$

where *STATUS* is the experimental dummy, β_0 is an intercept, X_x a baseline characteristic, and ε an error term. Table A.2 shows the results of an estimation of this equation, using ordinary least squares. In this equation the X_x vector was represented by the same 51 baseline characteristics that were used as covariates in the impact regressions done for this report. The F-test at the bottom of this table shows that the R^2 is not significantly different from zero, implying that there is no systematic relationship between sample characteristics at baseline and the experimental assignment variable. This, in turn, suggests that random assignment was effective.

---

[1] This is often referred to as a "multiple comparisons problem."

TABLE A.1

SELECTED CHARACTERISTICS OF THE NEW CHANCE SAMPLE AT RANDOM ASSIGNMENT, BY RESEARCH GROUP

Characteristic and Subgroup at Random Assignment	Experimentals	Controls	Full Sample	p (a)
Demographic characteristics				
Age (years) (%)				0.574
16	2.2	1.8	2.1	
17	18.2	16.1	17.5	
18	22.2	21.9	22.1	
19	24.9	27.1	25.6	
20	19.4	19.7	19.5	
21	10.7	11.9	11.1	
22	2.3	1.5	2.1	
Average age (years)	18.8	18.9	18.8	0.359
Ethnicity (%)				0.881
Black, non-Hispanic	52.0	53.3	52.4	
Hispanic	22.5	21.9	22.3	
White	22.8	22.5	22.7	
Other	2.7	2.2	2.5	
Marital status (%)				0.540
Never married	90.4	89.5	90.1	
Other	9.6	10.5	9.9	
Number of children (%)				0.779
1	65.5	63.8	64.9	
2	26.2	28.1	26.8	
3 or more	8.4	8.0	8.3	
Average number of children	1.4	1.5	1.4	0.805
Age of youngest child (years) (%)				0.804
Less than 1	53.8	53.7	53.8	
1	26.6	26.6	26.6	
2	12.4	11.5	12.1	
3 or older	7.2	8.2	7.5	
Average age of youngest child (years)	1.2	1.2	1.2	0.446
Age at first child's birth (years) (%)				0.820
14 or under	5.6	4.5	5.3	
15	12.0	12.2	12.1	
16	23.2	22.4	22.9	
17	25.9	26.6	26.1	
18	21.4	21.8	21.6	
19	11.9	12.5	12.1	
Average age at first child's birth (years)	16.8	16.9	16.8	0.426

(continued)

TABLE A.1 (continued)

Characteristic and Subgroup at Random Assignment	Experimentals	Controls	Full Sample	p (a)
Living arrangement				
Living with (%)				
Mother	34.8	33.7	34.4	0.642
Father	7.5	7.8	7.6	0.861
Spouse or partner	11.8	11.2	11.6	0.696
No other adult	31.9	31.6	31.8	0.891
Lived in a female-headed household at age 14 (%)	48.1	50.6	48.9	0.562
Lived with both parents at age 14 (%)	22.4	21.9	22.2	0.811
Education characteristics				
Highest grade completed (%)				0.964
7th or below	3.1	2.6	3.0	
8th	10.1	10.6	10.3	
9th	23.4	21.7	22.8	
10th	29.8	31.4	30.3	
11th	27.9	27.7	27.8	
12th	5.8	5.9	5.8	
Average highest grade completed	9.9	9.9	9.9	0.708
Received high school diploma or GED (%)	6.5	5.9	6.3	0.572
Left school before first pregnancy (%)	37.1	37.6	37.2	0.825
Average number of years since last attended school	2.4	2.4	2.4	0.589
Reading level (grade equivalent) (%)				
4th grade or below	8.3	8.9	8.5	0.630
5th grade	5.5	6.4	5.8	
6th grade	10.2	7.6	9.4	
7th grade	11.0	10.2	10.8	
8th grade	14.0	14.0	14.0	
9th grade	21.2	21.8	21.4	
10th grade or above	29.7	31.1	30.1	
Average reading level (grade equivalent)	8.3	8.5	8.4	0.356
Desired educational attainment for self (%)				0.801
High school diploma or GED	32.7	32.5	32.6	
1-3 years of college (A.A. degree)	30.8	32.1	31.2	
4 years of college (B.A. degree)	22.2	22.3	22.2	
Graduate degree	10.8	10.6	10.8	
Other	3.5	2.5	3.2	

(continued)

TABLE A.1 (continued)

Characteristic and Subgroup at Random Assignment	Experimentals	Controls	Full Sample		p (a)
Desired educational attainment for child (b) (%)					0.323
Elementary school	0.1	0.4	0.2		
High school	20.6	21.1	20.8		
College/post-secondary	57.8	57.9	57.8		
Graduate school	21.5	20.6	21.2		
Mother has high school diploma or GED (%)	52.4	52.8	52.5		0.814
Mother attended college (%)	24.9	25.5	25.1		0.617
Father has high school diploma or GED (%)	41.7	46.1	43.1	**	0.039
Father attended college (%)	15.6	17.4	16.2		0.386
Both parents have high school diplomas or GEDs (%)	28.6	31.1	29.4		0.118
Both parents attended college (%)	6.9	8.1	7.3		0.584
Employment and welfare receipt					
Number of jobs ever held (%)					0.825
0	21.2	22.1	21.5		
1-2	33.5	31.1	32.7		
3 or more	45.3	46.9	45.8		
Average number of jobs held	4.0	4.2	4.1		0.771
Employed at random assignment (%)	2.7	4.0	3.1	*	0.093
Number of months employed in prior 12 months (%)					0.565
0	63.3	63.2	63.3		
3 or less	18.4	18.4	18.4		
4-6	10.0	10.0	10.0		
7-12	8.1	7.6	8.0		
Prior-year earnings (%)					0.253
$0-$500	79.2	81.3	79.9		
$501 or more	20.8	18.7	20.1		
Length of longest job (%)					0.524
Never employed	21.0	21.8	21.2		
Less than one 1 month	4.1	2.7	3.7		
1-3 months	22.9	22.3	22.7		
4-6 months	21.9	23.2	22.3		
7-12 months	18.3	17.1	17.9		
Over 1 year	11.8	13.0	12.2		
Mother employed (%)					0.419
Yes	50.0	49.3	49.8		
No	41.9	42.7	42.2		
Don't know	3.7	4.7	4.0		
Deceased	4.4	3.2	4.0		

(continued)

TABLE A.1 (continued)

Characteristic and Subgroup at Random Assignment	Experimentals	Controls	Full Sample	p (a)
Father employed (%)				0.697
Yes	44.8	46.5	45.4	
No	20.2	18.9	19.8	
Don't know	25.4	24.1	25.0	
Deceased	9.5	10.5	9.8	
Receives AFDC (%)				0.720
Own grant	87.0	88.2	87.4	
Other person's grant	7.7	6.8	7.4	
Not receiving AFDC	5.3	5.0	5.2	
Receives (%)				
Medicaid	86.6	88.0	87.1	0.604
Food stamps	83.7	83.5	83.7	0.778
Public housing	22.4	25.0	23.2	0.125
Income from a job	3.4	3.7	3.5	0.571
Family received AFDC when sample member was growing up (%)				* 0.096
Always	17.3	15.2	16.6	
2 years or less (c)	19.0	18.0	18.7	
More than 2 years, but not always (c)	26.8	32.0	28.5	
Never	36.9	34.8	36.2	
Fertility-related characteristics				
Number of pregnancies (%)				
1	43.8	42.4	43.4	0.218
2	30.7	35.3	32.2	
3	16.4	15.4	16.1	
4	6.5	4.9	6.0	
5 or more	2.6	2.1	2.4	
Average number of pregnancies	1.9	1.9	1.9	0.350
Ever had an abortion (%)	23.5	22.1	23.1	0.467
When next child is expected (%)				0.991
Not expecting another child	64.0	64.5	64.2	
Within 2 years	6.7	7.5	7.0	
In 2-4 years	12.0	10.9	11.6	
In 5 years or more	17.3	17.2	17.3	
Average number of years until next child is expected (d)	4.5	4.4	4.4	0.731
Current birth control use (%)				0.724
Yes, using birth control	62.9	61.3	62.4	
No, not using birth control	12.2	12.1	12.1	
No partner/not having sex	24.9	26.6	25.5	
Used birth control at last intercourse (%)	70.9	71.1	71.0	0.901

(continued)

TABLE A.1 (continued)

Characteristic and Subgroup at Random Assignment	Experimentals	Controls	Full Sample	p (a)
Relations with child's father				
Speaks with child's father (b) (%)	67.5	67.6	67.5	0.980
Has child support order (b) (%)	28.0	27.6	27.9	0.841
Prior and current service receipt				
Ever in occupational skills training (%)	22.1	22.8	22.3	0.695
Services received in the 60 days before random assignment (%)				
Health care for child	84.7	84.4	84.6	0.880
Family planning	24.0	20.7	22.9	* 0.091
Mental health	2.8	2.5	2.7	0.735
Health care for self	59.3	57.4	58.7	0.396
Parenting	11.3	10.4	11.0	0.550
Life skills	2.9	3.0	2.9	0.882
Counseling	4.4	3.4	4.1	0.311
Other services	10.4	10.7	10.5	0.847
No services	8.4	8.5	8.4	0.967
Has regular child care (e) (%)	44.8	41.4	43.7	0.142
Psychosocial characteristics				
CES–D (depression) Scale (f) (%)				0.437
0–15 (not at risk)	47.8	45.4	47.0	
16–23 (at some risk)	25.1	27.5	25.9	
24–60 (at high risk)	27.2	27.1	27.2	
Average CES–D score (f)	17.9	18.3	18.1	0.382
Average number of sources of emotional support	2.8	2.7	2.7	0.448
Average level of satisfaction with emotional support (g)	4.2	4.2	4.2	0.869
Average self–esteem score (h)	38.3	38.3	38.3	0.879
Average Locus of Control score (i)	22.0	22.0	22.0	0.953
Other				
Has home telephone (%)	84.2	83.4	83.9	0.615
Has driver's license (%)	28.1	27.4	27.9	0.745
Sample size	1,408	680	2,088	

(continued)

TABLE A.1 (continued)

SOURCE: MDRC calculations from New Chance Enrollment Form data.

NOTES: Calculations for this table used data for all 2,088 sample members for whom there were 18 months of follow-up survey data, including those with values of zero for outcomes and New Chance enrollees (i.e., experimentals) who did not participate in the program. The reported sample sizes may fall short of this number because of missing or unusable items from some sample members' questionnaires.

(a) A t-test or F-test was applied to each difference in characteristics between research groups. The column labeled "p" is the statistical significance level of these differences: That is, p is the probability that these differences exist only because of random error. Statistical significance levels are indicated as *** = 1 percent; ** = 5 percent; * = 10 percent.

(b) When a sample member had more than 1 child, her response refers to her first child.

(c) The family's AFDC receipt may not have been continuous.

(d) Includes only those sample members who expected to have more children.

(e) Regular child care was defined as an ongoing arrangement used while the mother was in school, in training, or working.

(f) The Center for Epidemiological Studies Depression (CES-D) Scale is a widely used measure of depression; scores can range from zero to 60.

(g) Enrollees were also asked about their degree of satisfaction with the emotional support ("people who listen to you, reassure you, and show you they care") they received. Levels range from 1 (very dissatisfied) to 5 (very satisfied).

(h) The measure of self-esteem used was the Rosenberg Self-Esteem Scale, a 10-item scale that assesses a person's global sense of self-worth. Scores can range from 10 to 50; 30 is considered the neutral midpoint.

(i) The Locus of Control Scale is a six-item adaptation of the longer scale originally developed by Julien Rotter (1966). Scores can range from 6 to 30; 18 is considered the neutral midpoint.

TABLE A.2

ESTIMATED REGRESSION COEFFICIENTS FOR THE PROBABILITY OF ASSIGNMENT TO THE EXPERIMENTAL GROUP

Variable (a)	Parameter Estimate	Standard Error	p (b)
Constant	0.658 ***	0.056	0.000
Allentown	−0.040	0.063	0.526
Bronx	0.004	0.062	0.954
Chicago Heights	−0.020	0.071	0.781
Chula Vista	−0.040	0.067	0.550
Denver	−0.033	0.068	0.629
Detroit	−0.015	0.057	0.797
Harlem	0.007	0.061	0.907
Inglewood	−0.015	0.060	0.805
Jacksonville	−0.017	0.060	0.784
Lexington	0.044	0.060	0.469
Minneapolis	−0.003	0.061	0.956
Philadelphia	−0.029	0.059	0.626
Portland	−0.043	0.061	0.481
Salem	−0.017	0.068	0.803
San Jose	−0.028	0.065	0.672
Age 20−22	−0.024	0.039	0.539
More than 1 child	−0.045	0.030	0.133
Age 16 or younger when first child was born	0.026	0.026	0.312
Ever had an abortion	−0.003	0.030	0.909
Has a driver's license	0.011	0.027	0.694
Ever had a miscarriage	0.009	0.033	0.786
Has no home phone	−0.014	0.030	0.643
Hispanic	0.028	0.035	0.421
Not black or Hispanic	0.031	0.034	0.358
Highest grade completed is above 10th	0.002	0.025	0.951
Has a high school diploma or GED	0.006	0.052	0.904
TABE grade level is below 8th grade (c)	0.032	0.022	0.158
Ever had vocational/occupational skills training	−0.008	0.026	0.765
Highest educational goal is high school/GED or other	0.011	0.023	0.638
Receives child support from first child's father	0.012	0.025	0.619
Did not use birth control when last had sex	0.002	0.024	0.942
Has regular child care (d)	0.029	0.022	0.196
Was pregnant more than twice	0.058 *	0.035	0.097
Youngest child is older than 1 year old	−0.007	0.024	0.777
Locus of Control score is less than 21 (e)	−0.002	0.024	0.939
Self−esteem score is less than 35 (f)	0.029	0.026	0.266
Ever repeated a grade	−0.009	0.023	0.703
Not receiving AFDC in own name	0.004	0.036	0.920
Heard about New Chance from welfare officer	−0.033	0.027	0.235
Not JOBS−mandatory	0.034	0.040	0.394
At risk of depression	−0.034	0.023	0.142
Currently or previously married	−0.029	0.037	0.431

(continued)

TABLE A.2 (continued)

Variable (a)	Parameter Estimate	Standard Error	p (b)
Family never on welfare when sample member growing up	0.010	0.023	0.662
Public housing	−0.042	0.028	0.126
Received family planning services in the prior 60 days	0.045 *	0.026	0.087
First child's father never sees child	0.005	0.023	0.844
Does not expect to have more children	−0.005	0.023	0.840
Lived with father at age 14	−0.001	0.025	0.959
Ever employed in prior 12 months	−0.037	0.031	0.237
Earned $501 or more in prior 12 months	0.052	0.035	0.140
Never employed	−0.017	0.029	0.568
Sample size	2,088		
Number of experimentals	1,408		
Number of controls	680		
Mean of dependent variable	0.658		
R−square	0.014		
F−statistic	0.563		
P−value of F−statistic	0.995		

SOURCE: MDRC calculations from the New Chance Enrollment Form data.

NOTES: The dependent variable in each regression equation was unity for each experimental and zero for each control. Each characteristic on the right−hand side of each equation was measured as a deviation from its mean.

The p−value of the F−statistic is the probability of obtaining these coefficient estimates if the true chance of becoming an experimental did not vary with any characteristic. Thus, the closer the p−value is to unity, the more successful was random assignment in equating average characteristics of experimentals and controls.

(a) No dummy variable for Pittsburgh was included, as this would overdetermine the regression model.

(b) A two−tailed t−test was applied to each coefficient estimate. The column labeled "p" indicates the statistical significance level of the coefficient: That is, p is the probability that the actual value coefficient is zero. Statistical significance levels are indicated as *** = 1 percent; ** = 5 percent; * = 10 percent.

(c) The test administered was the reading part of the Tests of Adult Basic Education (TABE), Survey Form, a 30−item test of reading vocabulary and reading comprehension.

(d) Regular child care was defined as an ongoing arrangement used while the mother was in school, in training, or working.

(e) The Locus of Control Scale is a six−item adaptation of the longer scale originally developed by Julien Rotter (1966). Scores can range from 6 to 30; 18 is considered the neutral midpoint.

(f) The measure of self−esteem used was the Rosenberg Self−Esteem Scale, a 10−item scale that assesses a person's global sense of self−worth. Scores can range from 10 to 50; 30 is considered the neutral midpoint.

APPENDIX B

ANALYSIS OF SURVEY NONRESPONSE

APPENDIX B

ANALYSIS OF SURVEY NONRESPONSE

This appendix compares the New Chance Wave I survey respondent sample with the entire sample of those who were randomly assigned. As discussed in Chapter 2, the primary difference between these samples is that the survey respondent sample does not include sample members who applied for New Chance and were randomly assigned but for whom no usable follow-up data were available. These sample members were treated as survey nonrespondents and excluded from the impact analyses. This appendix explores the extent to which the survey respondent samples are representative of the full New Chance study sample. In summary, it shows that the response rate on the 18-month follow-up survey was quite high, resulting in a study sample that is a good representation of the overall sample of randomly assigned young women. Differences between respondents and nonrespondents are small and often not statistically significant.

Table B.1 shows survey response rates by research status and site. The overall response rate was close to 90 percent, which is generally considered very high in a population such as this one. Experimentals were somewhat more likely to respond to the follow-up survey than controls, and there was some variation in response rates across the sites. Harlem had the lowest response rate (82.7 percent) and the response rate was highest in Detroit, where outcome data were collected for 96.6 percent of all sample members.

Throughout the report, five different respondent samples were used. As discussed in Chapter 2, certain outcome variables were missing or invalid for sample members who did answer most of the other questions. To maximize the sample sizes for individual chapters, five respondent samples were created, including all sample members for whom a set of relevant measures was present.[1] As expected, these samples have a large degree of overlap. They all exclude those cases for whom no survey data were available at all. These samples were introduced in Chapter 2 and four of them are included in a comparison of baseline characteristics in Table B.2. Asterisks in this table indicate the statistical significance of the difference between each of these survey samples and the full New Chance sample. It appears that, in general, differences among the samples are small and not systematic.

To further explore the issue of nonresponse, a multivariate analysis was conducted, which uses the experimental assignment variable, site variables, and 36 other baseline characteristics to identify the correlates of survey response. Table B.3 shows the results of this analysis. The parameter estimates in the first column capture the effect of each variable on the probability of being in the primary Wave I study sample. The asterisks and p-values show the statistical significance of this relationship. The F-statistic and its p-value at the bottom of the table show that these differences were statistically significant overall. Nevertheless, the low R^2 (0.041) suggests that baseline characteristics and the experimental assignment do not discriminate very well between item-survey respondents and nonrespondents. Consequently, the survey sample may be considered adequately representative of the full New Chance study sample.

---

[1] The first sample (Sample 1) consisted of 2,088 respondents for whom valid data were available on most outcomes in the report. The next sample (Sample 2) included 2,024 respondents (most of whom were also in Sample 1), for whom pregnancy, birth, and health outcomes were available. Sample 3 (2,046 respondents) included those for whom TABE literacy scores were collected, Sample 4 (1,842 focal children) was used to measure parenting and home environment outcomes, and Sample 5 (1,932 focal children) was used for the study of child care outcomes.

TABLE B.1

SURVEY RESPONSE RATES, BY RESEARCH GROUP AND SITE

Research Group and Site	Sample Size	Survey Response Rate (%)		p
Full sample	2,322	89.9		
Research group			*	0.092
Experimental	1,553	90.7		
Control	769	88.4		
Site			***	0.000
Allentown	124	92.7		
Bronx	150	84.0		
Chicago Heights	78	88.5		
Chula Vista	150	84.7		
Denver	123	89.4		
Detroit	175	96.6		
Harlem	150	82.7		
Inglewood	146	89.7		
Jacksonville	154	93.5		
Lexington	150	90.0		
Minneapolis	127	95.3		
Philadelphia	150	90.0		
Pittsburgh	180	95.0		
Portland	166	86.1		
Salem	150	89.3		
San Jose	150	89.3		

SOURCE: MDRC calculations from New Chance Enrollment Form and 18-month survey data.

NOTES: A t-test or F-test was applied to the difference in response rates among research groups and sites. The column labeled "p" is the statistical significance level of this difference: That is, p is the probability that this difference exists only because of random error. Statistical significance levels are indicated as *** = 1 percent; ** = 5 percent; * = 10 percent.

TABLE B.2

SELECTED CHARACTERISTICS OF THE NEW CHANCE SAMPLE AT RANDOM ASSIGNMENT, BY SURVEY RESPONDENT SUBSAMPLE

Characteristic and Subgroup at Random Assignment	Full Sample	In Sample 1	In Sample 2	In Sample 3	In Sample 4
Demographic characteristics					
Age (years) (%)					
16	2.1	2.1	2.1	2.2	2.2
17	17.3	17.5	17.3	17.5	17.1
18	21.9	22.1	22.5	22.2	22.7
19	25.6	25.6	25.4	25.7	25.5
20	20.0	19.5	19.6	19.4	19.1
21	11.1	11.1	11.1	11.1	11.5
22	1.9	2.1	2.0	2.0	2.0
Average age (years)	18.9	18.8	18.8	18.8	18.8
Ethnicity (%)					*
Black, non-Hispanic	52.2	52.4	52.2	52.8	53.2
Hispanic	22.3	22.3	22.5	22.5	22.6
White	22.9	22.7	22.8	22.3	22.0
Other	2.5	2.5	2.5	2.4	2.2
Marital status (%)				*	**
Never married	90.1	90.1	89.9	90.5	90.7
Other	9.9	9.9	10.1	9.5	9.3
Number of children (%)					
1	65.0	64.9	65.0	65.3	65.4
2	26.8	26.8	26.8	26.5	26.4
3 or more	8.2	8.3	8.2	8.2	8.2
Average number of children	1.4	1.4	1.4	1.4	1.4
Age of youngest child (years) (%)					
Less than 1	53.5	53.8	53.8	53.7	54.2
1	27.1	26.6	26.8	26.7	26.6
2	11.7	12.1	12.1	12.0	11.6
3 or older	7.7	7.5	7.4	7.6	7.6
Average age of youngest child (years)	1.2	1.2	1.2	1.2	1.2
Age at first child's birth (years) (%)					
14 or under	5.3	5.3	5.2	5.3	5.3
15	12.1	12.1	11.9	11.9	11.6
16	23.0	22.9	23.0	23.1	22.6
17	26.3	26.1	26.1	26.2	26.6
18	21.1	21.6	21.6	21.6	22.0
19	12.3	12.1	12.1	11.8	11.9
Average age at first child's birth (years)	16.8	16.8	16.8	16.8	16.8

(continued)

TABLE B.2 (continued)

Characteristic and Subgroup at Random Assignment	Full Sample	In Sample 1	In Sample 2	In Sample 3	In Sample 4
Living arrangement					
Living with (%)					
Mother	33.9	34.4	34.6 *	34.6 *	35.3 ***
Father	7.9	7.6	7.8	7.7	7.8
Spouse or partner	11.5	11.6	11.7	11.1	11.2
No other adult	32.2	31.8	31.6	32.0	31.9
Lived in a female-headed household at age 14 (%)	40.0	48.9 **	48.7 **	49.0 **	49.6
Lived with both parents at age 14 (%)	22.6	22.2	22.3	22.1	22.2
Education characteristics					
Highest grade completed (%)					*
7th or below	3.0	3.0	3.0	2.9	2.9
8th	10.6	10.3	10.2	10.4	10.4
9th	22.9	22.8	22.7	22.7	22.2
10th	30.6	30.3	30.4	30.4	30.7
11th	27.4	27.8	27.9	27.8	28.1
12th	5.4	5.8	5.8	5.8	5.9
Average highest grade completed	9.8	9.9 ***	9.9 **	9.9 **	9.9 ***
Received high school diploma or GED (%)	5.9	6.3 **	6.3 **	6.2 *	6.2
Left school before first pregnancy (%)	38.3	37.2 ***	37.2 ***	37.1 ***	37.5
Average number of years since last attended school	2.4	2.4 ***	2.4 ***	2.4 ***	2.3 ***
Reading level (grade equivalent) (%)					
4th grade or below	9.0	8.5	8.5	8.7	8.5
5th grade	5.9	5.8	5.9	5.7	5.6
6th grade	9.2	9.4	9.2	9.5	9.2
7th grade	10.8	10.8	10.9	10.9	10.8
8th grade	14.0	14.0	13.8	14.3	14.3
9th grade	20.9	21.4	21.5	21.2	21.4
10th grade or above	30.2	30.1	30.3	29.6	30.2
Average reading level (grade equivalent)	8.4	8.4	8.4	8.3	8.4
Desired educational attainment for self (%)					
High school diploma or GED	33.1	32.6	32.6	32.6	32.8
1-3 years of college (A.A. degree)	30.6	31.2	31.2	31.2	31.3
4 years of college (B.A. degree)	22.1	22.2	22.4	22.0	22.0
Graduate degree	10.8	10.8	10.6	10.8	10.6
Other	3.3	3.2	3.3	3.3	3.3

(continued)

TABLE B.2 (continued)

Characteristic and Subgroup at Random Assignment	Full Sample	In Sample 1	In Sample 2	In Sample 3	In Sample 4
Desired educational attainment for child (%) (a)				**	
Elementary school	0.2	0.2	0.2	0.2	0.2
High school	21.3	20.8	20.9	20.5	20.5
College/post-secondary	57.3	57.8	57.7	58.3	58.0
Graduate school	21.3	21.2	21.2	20.9	21.4
Mother has high school diploma or GED (%)	52.2	52.5	52.5	52.6	52.8
Mother attended college (%)	25.5	25.1	25.3	25.2	25.4
Father has high school diploma or GED (%)	42.6	43.1	42.9	43.3 **	43.1
Father attended college (%)	16.0	16.2	16.3	16.2	16.0
Both parents have high school diplomas or GEDs (%)	28.9	29.4 *	29.2	29.6 **	29.4
Both parents attended college (%)	7.6	7.3	7.4	7.1 **	7.0 **
Employment and welfare receipt					
Number of jobs ever held (%)					
0	21.7	21.5	21.3	21.4	21.2
1-2	32.6	32.7	32.9	32.9	32.7
3 or more	45.8	45.8	45.7	45.7	46.1
Average number of jobs held	4.2	4.1	4.1	4.1	4.2
Employed at random assignment (%)	3.1	3.1	3.0	3.2	3.1
Number of months employed in the prior 12 months (%)					
0	63.4	63.3	63.3	63.0	62.8
3 or less	18.7	18.4	18.5	18.6	18.9
4-6	10.0	10.0	10.0	9.9	10.0
7-12	7.6	8.0	7.7	8.0	8.0
Prior-year earnings (%)					
$0-$500	80.2	79.9	79.9	79.7	79.9
$501 or more	19.8	20.1	20.1	20.3	20.1
Length of longest job (%)		*			
Never employed	21.5	21.2	21.1	21.2	21.0
Less than one 1 month	4.0	3.7	3.7	3.7	3.7
1-3 months	22.7	22.7	22.9	22.7	22.9
4-6 months	22.3	22.3	22.2	22.3	22.6
7-12 months	17.6	17.9	17.9	17.9	17.8
Over 1 year	11.9	12.2	12.2	12.2	12.0

(continued)

TABLE B.2 (continued)

Characteristic and Subgroup at Random Assignment	Full Sample	In Sample 1	In Sample 2	In Sample 3	In Sample 4
Mother employed (%)		**	*		
Yes	49.3	49.8	49.8	50.0	49.6
No	42.1	42.2	42.1	41.8	42.5
Don't know	4.4	4.0	4.0	4.2	4.1
Deceased	4.2	4.0	4.2	4.1	3.8
Father employed (%)					
Yes	45.1	45.4	45.4	45.5	46.2
No	19.9	19.8	19.9	19.4	19.2
Don't know	25.2	25.0	24.9	25.3	24.8
Deceased	9.8	9.8	9.9	9.8	9.8
Receives AFDC (%)		**		**	
Own grant	87.2	87.4	87.2	87.1	87.0
Other person's grant	7.2	7.4	7.4	7.6	7.7
Not receiving AFDC	5.6	5.2	5.4	5.3	5.3
Receives (%)					
Medicaid	87.3	87.1	86.9 *	87.0	86.9
Food stamps	83.8	83.7	83.6	83.7	83.7
Public housing	23.4	23.2	23.4	23.7	24.1 *
Income from a job	3.5	3.5	3.3	3.5	3.4
Family received AFDC when sample member was growing up (%)					
Always	16.7	16.6	16.3	16.7	17.2
2 years or less (b)	18.4	18.7	18.9	18.6	18.3
More than 2 years, but not always (b)	28.3	28.5	28.5	28.6	28.3
Never	36.6	36.2	36.4	36.1	36.2
Fertility–related characteristics					
Number of pregnancies (%)					
1	43.2	43.4	43.8	43.6	43.5
2	32.3	32.2	31.6	32.2	32.3
3	16.2	16.1	16.3	16.1	15.9
4	6.0	6.0	6.0	5.7	5.8
5 or more	2.3	2.4	2.3	2.3	2.5
Average number of pregnancies	1.9	1.9	1.9	1.9	1.9
Ever had an abortion (%)	76.9	23.1	22.6	23.1	23.7
When next child is expected (%)					**
Not expecting another child	64.5	64.2	63.9	64.2	63.2
Within 2 years	6.9	7.0	7.0	6.8	6.5
In 2–4 years	11.6	11.6	11.7	11.6	12.1
In 5 years or more	17.1	17.3	17.4	17.4	18.2
Average number of years until next child is expected (c)	4.4	4.4	4.5	4.5 **	4.5 **

(continued)

TABLE B.2 (continued)

Characteristic and Subgroup at Random Assignment	Full Sample	In Sample 1	In Sample 2	In Sample 3	In Sample 4
Current birth control use (%)		*			
Yes, using birth control	61.5	62.4	62.2	62.4	62.5
No, not using birth control	12.4	12.1	12.1	12.0	11.9
No partner/not having sex	26.1	25.5	25.8	25.6	25.7
Used birth control at last intercourse (%)	70.5	71.0	70.7	70.8	71.0
Relations with child's father					
Speaks with child's father (a) (%)	66.6	67.5 ***	67.1	67.6 ***	67.9 ***
Has child support order (a) (%)	27.2	27.9 **	28.0 **	28.0 **	27.6
Prior and current service receipt					
Ever in occupational skills training (%)	22.4	22.3	22.3	22.5	22.1
Services received in the 60 days before random assignment (%)					
Health care for child	84.5	84.6	84.9	84.5	85.2 *
Family planning	23.2	22.9	22.8	22.8	23.3
Mental health	2.8	2.7	2.6 *	2.7	2.7
Health care for self	59.0	58.7	58.6	58.4	58.7
Parenting	11.3	11.0	11.1	10.9	10.9
Life skills	3.1	2.9 *	3.0	2.8 **	2.6 ***
Counseling	4.1	4.1	4.1	4.0	3.8
Other services	10.3	10.5	10.7 *	10.4	10.1
No services	8.3	8.4	8.1	8.6	8.5
Has regular child care (d) (%)	44.2	43.7	43.8	43.6	43.4
Psychosocial characteristics					
CES-D (depression) Scale (e) (%)		*	*		
0-15 (not at risk)	46.5	47.0	46.9	46.7	47.0
16-23 (at some risk)	25.7	25.9	26.0	26.0	26.0
24-60 (at high risk)	27.8	27.2	27.1	27.3	27.0
Average CES-D score (e)	18.1	18.1	18.1	18.1	18.0
Average number of sources of emotional support	2.8	2.7	2.8	2.7	2.8
Average level of satisfaction with emotional support (f)	4.2	4.2	4.2	4.2	4.2
Average self-esteem score (g)	38.3	38.3	38.4	38.4	38.4
Average Locus of Control score (h)	22.0	22.0	22.0	22.0	22.0
Other					
Has home telephone (%)	83.7	83.9	83.9	84.1 *	84.5 **
Has driver's license (%)	27.3	27.9 *	28.0 **	27.4	27.7
Sample size	2,322	2,088	2,024	2,046	1,842

(continued)

TABLE B.2 (continued)

SOURCE: MDRC calculations from New Chance Enrollment Form data.

NOTES: Calculations for this table used data for all 2,322 sample members at baseline. The reported sample sizes may fall short of this number because of missing or unusable items from some sample members' questionnaires.

A t-test or F-test was applied to each difference in characteristics between survey respondent subsamples. Statistical significance levels are indicated as *** = 1 percent; ** = 5 percent; * = 10 percent.

(a) When a sample member had more than 1 child, her response refers to her first child.

(b) The family's AFDC receipt may not have been continuous.

(c) Includes only those sample members who expected to have more children.

(d) Regular child care was defined as an ongoing arrangement used while the mother was in school, in training, or working.

(e) The Center for Epidemiological Studies Depression (CES-D) Scale is a widely used measure of depression; scores can range from zero to 60.

(f) Enrollees were also asked about their degree of satisfaction with the emotional support ("people who listen to you, reassure you, and show you they care") they received. Levels range from 1 (very dissatisfied) to 5 (very satisfied).

(g) The measure of self-esteem used was the Rosenberg Self-Esteem Scale, a 10-item scale that assesses a person's global sense of self-worth. Scores can range from 10 to 50; 30 is considered the neutral midpoint.

(h) The Locus of Control Scale is a six-item adaptation of the longer scale originally developed by Julien Rotter (1966). Scores can range from 6 to 30; 18 is considered the neutral midpoint.

TABLE B.3

ESTIMATED REGRESSION COEFFICIENTS FOR THE PROBABILITY OF BEING A RESPONDER TO THE 18–MONTH SURVEY

Variable (a)	Parameter Estimate	Standard Error	p (b)
Constant	89.922 ***	0.619	0.000
Experimental assignment dummy	2.366 *	1.324	0.074
Allentown	−2.185	3.866	0.572
Bronx	−10.646 ***	3.715	0.004
Chicago Heights	−3.948	4.288	0.357
Chula Vista	−12.668 ***	3.971	0.001
Denver	−7.097 *	4.091	0.083
Detroit	4.010	3.497	0.252
Harlem	−10.244 ***	3.639	0.005
Inglewood	−4.237	3.614	0.241
Jacksonville	1.042	3.678	0.777
Lexington	−3.473	3.657	0.342
Minneapolis	1.251	3.752	0.739
Philadelphia	−2.051	3.607	0.570
Portland	−10.408 ***	3.650	0.004
Salem	−6.026	4.084	0.140
San Jose	−8.729 **	3.937	0.027
Age 20–22	1.719	2.325	0.460
More than 1 child	0.848	1.799	0.638
Age 16 or younger when first child was born	−1.109	1.523	0.466
Ever had an abortion	0.424	1.768	0.811
Has a driver's license	2.172	1.643	0.186
Ever had a miscarriage	−1.448	1.980	0.465
Has no home phone	−0.511	1.767	0.772
Hispanic	5.742 ***	2.095	0.006
Not black or Hispanic	1.926	2.002	0.336
Highest grade completed is above 10th	1.925	1.510	0.202
Has a high school diploma or GED	10.221 ***	3.172	0.001
TABE grade level is below 8th (c)	−2.531 *	1.344	0.060
Ever had vocational/occupational skills training	−0.674	1.558	0.665
Highest educational goal is high school/GED or other	−1.237	1.357	0.362
Receives child support from first child's father	1.454	1.505	0.334
Did not use birth control when last had sex	−1.545	1.415	0.275
Has regular child care (d)	−1.307	1.351	0.333
Was pregnant more than twice	0.642	2.084	0.758
Youngest child is older than 1 year old	−0.728	1.440	0.613
Locus of Control score is less than 21 (e)	−0.517	1.416	0.715
Self–esteem score is less than 35 (f)	−0.746	1.570	0.635
Ever repeated a grade	1.353	1.376	0.326
Not receiving AFDC in own name	0.290	2.188	0.895
Heard about New Chance from welfare officer	0.628	1.640	0.702
Not JOBS–mandatory	−5.513 **	2.355	0.019
At risk of depression	−1.439	1.377	0.296
Currently or previously married	1.256	2.230	0.573

(continued)

TABLE B.3 (continued)

Variable (a)	Parameter Estimate	Standard Error	p (b)
Family never on welfare when sample member growing up	-1.692	1.384	0.222
Public housing	-1.502	1.649	0.362
Received family planning services in prior 60 days	-1.175	1.558	0.451
First child's father never sees child	-1.110	1.393	0.425
Does not expect to have more children	-1.754	1.352	0.195
Lived with father at age 14	-2.529 *	1.492	0.090
Ever employed in prior 12 months	-1.459	1.851	0.431
Earned $501 or more in prior 12 months	2.143	2.104	0.309
Never employed	-0.698	1.732	0.687
Sample size	2,322		
Mean of dependent variable	89.922		
R-square	0.041		
F-statistic	1.886		
P-value of F-statistic	0.000		

SOURCE: MDRC calculations from the New Chance Enrollment Form data.

NOTES: The dependent variable in each regression equation was unity for each responder and zero for each non-responder. Each characteristic on the right-hand side of each equation was measured as a deviation from its mean. The standard error of each coefficient estimate is enclosed in parentheses.

The p-value of the F-statistic is the probability of obtaining these coefficient estimates if the true chance of responding did not vary with any characteristic. Thus, the closer the p-value is to unity, the more successful was random assignment in equating average characteristics of experimentals and controls.

(a) No dummy variable for Pittsburgh was included, as this would overdetermine the regression model.

(b) A t-test was applied to each coefficient estimate. The column labeled "p" indicates the statistical significance level of the coefficient: That is, p is the probability that the true coefficient is zero. Statistical significance levels are indicated as *** = 1 percent; ** = 5 percent; * = 10 percent.

(c) The test administered was the reading part of the Tests of Adult Basic Education (TABE), Survey Form, a 30-item test of reading vocabulary and reading comprehension.

(d) Regular child care was defined as an ongoing arrangement used while the mother was in school, in training, or working.

(e) The Locus of Control Scale is a six-item adaptation of the longer scale originally developed by Julien Rotter (1966). Scores can range from 6 to 30; 18 is considered the neutral midpoint.

(f) The measure of self-esteem used was the Rosenberg Self-Esteem Scale, a 10-item scale that assesses a person's global sense of self-worth. Scores can range from 10 to 50; 30 is considered the neutral midpoint.

APPENDIX C

THE ENVIRONMENT OF NEW CHANCE SITES

TABLE C.1

THE ENVIRONMENT OF NEW CHANCE SITES

Site/Metropolitan Area	1990 Population	1989 Percentage of Population Black	Hispanic (a)	1987 Percent of Total Births to Mothers Under Age 20	1987 Infant Death Rate (b)	1989 Crime Rate (c)
United States	248,710,000	12.2	8.8	12.4	10.1	5,741
Allentown/Allentown–Bethlehem MSA	686,688	1.7	2.8	9.0	8.5	N/A
Bronx/New York PMSA	8,546,846	26.3	21.9	10.0	12.1	8,861
Chicago Heights/Chicago PMSA	6,069,974	23.8	11.9	12.5	13.0	N/A
Chula Vista/San Diego MSA	2,498,016	5.9	18.6	9.7	9.4	7,362
Denver/Denver PMSA	1,622,980	5.3	11.5	10.2	10.4	6,593
Detroit/Detroit PMSA	4,382,299	21.0	1.9	12.0	11.9	6,975
Harlem/New York PMSA	8,546,846	26.3	21.9	10.0	12.1	8,861
Inglewood/Los Angeles–Long Beach PMSA	8,863,164	13.4	36.8	11.5	9.8	7,424
Jacksonville/Jacksonville MSA	906,727	22.2	2.2	14.3	10.9	8,854
Lexington/Lexington–Fayette MSA	348,428	11.7	0.8	14.0	8.5	5,727
Minneapolis/Minneapolis–St. Paul MSA	2,464,124	2.5	1.1	7.1	8.7	5,621
Philadelphia/Philadelphia PMSA	4,856,881	19.5	2.8	11.3	11.7	4,687
Pittsburgh/Pittsburgh PMSA	2,056,705	8.3	0.6	9.4	10.0	3,394
Portland/Portland PMSA	1,239,842	2.9	2.4	9.5	10.4	7,402
Salem/Salem MSA	278,024	0.5	5.2	12.5	11.3	6,247
San Jose/San Jose PMSA	1,497,577	3.7	21.7	7.7	8.5	4,839

(continued)

TABLE C.1 (continued)

Site/Metropolitan Area	1987 Money Income per Capita ($)	1988 Civilian Unemployment Rate (%)	1989 Civilian Unemployment Rate (%)	1989 Average Annual Pay (d) ($)	January 1990 Maximum AFDC Benefits for 3–Person Family ($)
United States	11,923	5.5	5.3	22,563	364 (e)
Allentown/Allentown–Bethlehem MSA	12,216	4.4	4.4	22,128	421
Bronx/New York PMSA	13,714	4.4	5.4	31,621	577
Chicago Heights/Chicago PMSA	13,338	6.2	5.5	26,341	367
Chula Vista/San Diego MSA	12,764	4.3	3.9	22,956	694
Denver/Denver PMSA	13,775	6.0	5.4	24,169	356
Detroit/Detroit PMSA	13,367	7.7	7.1	27,315	516
Harlem/New York PMSA	13,714	4.4	5.4	31,621	577
Inglewood/Los Angeles–Long Beach PMSA	13,357	4.9	4.7	26,716	694
Jacksonville/Jacksonville MSA	11,640	5.3	5.7	20,571	294
Lexington/Lexington–Fayette MSA	11,611	4.8	3.8	19,887	228
Minneapolis/Minneapolis–St. Paul MSA	14,340	3.4	3.8	24,372	532
Philadelphia/Philadelphia PMSA	13,064	4.1	3.8	24,914	421
Pittsburgh/Pittsburgh PMSA	11,785	5.7	4.6	22,530	421
Portland/Portland PMSA	12,336	4.8	4.5	22,228	432
Salem/Salem MSA	10,255	5.8	5.9	18,124	432
San Jose/San Jose PMSA	16,086	4.0	3.8	30,656	694

(continued)

TABLE C.1 (continued)

SOURCES: U.S. Bureau of the Census, 1991, 1993; U.S. Congress, House Committee on Ways and Means, 1990.

NOTES: (a) Hispanics of any race.
(b) Deaths of infants under 1 year old per 1,000 live births.
(c) Serious crimes (murder and non-negligent manslaughter, forcible rape, robbery, aggravated assault, burglary, larceny-theft, and motor vehicle theft) known to police, per 100,000 resident population.
(d) Derived from quarterly tax reports submitted to state agencies by employers.
(e) Median for 50 states and the District of Columbia.

APPENDIX D

NEW CHANCE SITE PROFILES

Expectant and Parenting Youth Program
Private Industry Council of Lehigh Valley
Allentown, Pennsylvania

As the local JTPA operator, the Private Industry Council (PIC) of Lehigh Valley has provided occupational skills training programs to youth and adults in the Allentown/Easton/Bethlehem area since 1983. New Chance operated as part of the PIC's Expectant and Parenting Youth Program (EPYP), which was created in 1985 to serve 14- to 21-year-old pregnant and parenting women who are high school dropouts. Housed at the PIC's offices in a predominantly residential working-class area of Allentown, EPYP/New Chance used PIC services, including on-site vocational training programs, an IBM PALS (Principles of Alphabet Literacy System) computer learning center, on-site child care, work internship development, and job placement assistance. The program served an ethnically mixed group of black, Hispanic, and white young mothers. It tended to attract young women residing in Allentown, but participants also came to the program by bus, private transportation, or the agency's van from other cities in the Lehigh Valley.

EPYP offered many of the New Chance activities before joining the demonstration, and was nationally recognized as a strong provider of education and parenting services for adolescent parents. EPYP is approved by the state's Department of Education as an alternative school for pregnant and parenting teens. The on-site day care center is an integral part of the parenting component and offers daily opportunities for staff to work with the young women and their children. The day care staff are employees of EPYP and coordinate child care services and parenting instruction with other activities.

During the period under study, EPYP's staff expanded the program's focus on employment-related services by enhancing the career exploration and pre-employment skills activities, making work internships a year-round option, and connecting participants with vocational training programs. The program also added family planning classes. In addition, the program formed linkages with other agencies to provide services, including workshops offered by instructors from the Penn State Cooperative Extension and from Planned Parenthood.

EPYP/New Chance staff incorporated some services that made the site's program especially comprehensive and responsive to the teens' needs. Most notable were the on-site clinic held twice a month by the Visiting Nurse Association (VNA) and the group home operated by EPYP. The monthly VNA clinics provided a highly accessible setting for New Chance mothers and their children in which medical staff could treat acute problems, provide immunizations and well-care checkups, and highlight the importance of regular health care. The EPYP/New Chance group home, which could accommodate up to five families at one time, enabled the program to respond to housing emergencies and helped prepare young women for independent living. A "house mother," whose title was Life Skills Coordinator, reinforced in the home setting what the teens learned in their life skills, nutrition, and health classes.

The program's education component was refined and restructured over the course of the demonstration to allow EPYP's strong team of teachers to deliver a more individualized education program responsive to the diverse needs of the students. In addition to creating separate sections for GED preparation and basic skills remediation, staff brought in volunteer tutors and developed a literacy lab.

As the first demonstration participants neared readiness for skills training (usually defined as passing the GED exam and having completed all Phase I activities), a staff person was hired to handle the referral and course entry process, either at the PIC or at other area training vendors. This staff person, the program's employment specialist, also helped participants complete the skills training courses and find training-related jobs. When participants were ready for skills training, the specialist took on case management responsibilities for them on employment-related matters; personal issues continued to be handled by the participants' original case manager. For participants interested in and thought ready to sustain longer-term participation in Associate's degree training courses, the program emphasized assistance in enrolling at one of the area's community colleges.

The employment specialist also assumed responsibility for teaching the program's employability development courses and developing and monitoring work internships. Internships developed by the specialist were a key part of the program's strategy for enhancing participants' ability to make career choices and reinforce instruction in the employability development class, although many participants also held jobs in the PIC's Summer Youth Service Corps.

Despite a well-planned recruitment campaign and strong linkages to two county welfare departments, recruitment was a challenge. Information from local welfare departments on the number of potential New Chance eligibles in the area indicates that the program was recruiting from a relatively small pool. However, staff successfully drew in a large percentage of the eligible population.

Since the time period under study in this report, the EPYP/New Chance program has begun helping participants make the transition into skills training courses by allowing them to first "job shadow," i.e., to observe a worker in a vocational field related to the content of their courses in order to get a better sense of the work in the career they have chosen before they enter training. Other enhancements designed to increase attendance and retention include expanded orientation to the program, recommitment contracts for participants with poor attendance and quarterly progress reports for all participants, and the county welfare staff's more active role in goal planning and progress review sessions for program participants.

The EPYP/New Chance program was fully supported by funds from the Single Point of Contact (SPOC) Program (part of Pennsylvania's JOBS program). All New Chance participants at this site had to be eligible for and enrolled in SPOC. The teen parents in New Chance were considered voluntary JOBS participants in Lehigh and Northampton counties. SPOC continued to provide funding for the programmatic expansions that were made for EPYP's participation in the New Chance demonstration, allowing these changes to be institutionalized.

National Puerto Rican Forum, Inc.
Bronx, New York

The New Chance program operated by the National Puerto Rican Forum, Inc. (NPRF) was located in the South Bronx, a community with a national reputation for high rates of school dropout, youth unemployment, teen pregnancy, infant mortality, and drug-related injury and death. This Bronx agency, the flagship of a national network of community-based social service and advocacy organizations, has served this largely Latino neighborhood since 1978. A combination of city, state, and private funding has enabled NPRF to offer education, job skills training, and job placement services. In the fall of 1989, MDRC, the New York State Department of Social Services (DSS), and NPRF contracted to implement New Chance. The program operated as part of the Comprehensive Employment Opportunity Support Centers (CEOSC), a DSS initiative that serves AFDC recipients with children under age 6. Due to a loss of CEOSC funding, NPRF ceased operating its New Chance program in September 1993.

NPRF's on-site education (featuring computer-assisted instruction), clerical skills training, job-readiness instruction, and job placement services were strong before New Chance was implemented. However, New Chance required NPRF to strengthen services for the parenting and health components and to add career exploration and family planning activities as well as other training options. The modifications were made under the stewardship of NPRF's core New Chance staff – a project director, two case managers, and a parenting instructor.

Strong ties with community agencies helped this site to fully implement the New Chance model. While NPRF could provide child care on a temporary or emergency basis, it had to rely on nearby child care centers, family day care, and other providers to meet the child care needs of participants. Through Montefiore Hospital's Community Clinic and Bronx Lebanon Hospital, New Chance participants and their children received health care screenings and follow-up referrals for treatment. Staff from the municipal health department and Montefiore made classroom presentations on family planning, lead poisoning, and prevention of childhood injuries. In addition, state, city, and private agencies served as referral centers for child and family welfare issues. Finally, New Chance participants attended special conferences on career and education opportunities held at local colleges. Along with these agency ties, the site also built relationships with prospective employers in the community.

Child care problems, unstable housing arrangements, and physical abuse affected program attendance and retention, and GED attainment rates were lower than expected. Staff explored a variety of strategies to overcome these challenges, including home visits, increased personal counseling, and asking participants who had been in the program for a while to serve as mentors for new participants.

DSS and the New York City Human Resources Administration (HRA), the city welfare agency, both provided valuable financial support and referrals of eligible applicants. Local JOBS funds paid for participants' support services and training-related expenses.

Aunt Martha's Youth Service Center, Inc. Chicago Heights, Illinois (currently located in Park Forest)

Aunt Martha's Youth Service Center, Inc., a 21-year-old community-based youth organization, offers comprehensive services to young people and their families living in Cook and Will counties, south of Chicago. Begun as a counseling center, Aunt Martha's has expanded to offer education, employment and health services, child care, legal assistance, youth development activities, and foster care services.

Aunt Martha's adopted the New Chance model in 1986, when it became one of six agencies selected to participate in the national pilot phase of the program. New Chance was built on the agency's existing parenting, family planning, and employment services. Some services, including education, were expanded for both the pilot phase and the demonstration. Child care was provided off-site through a network of day care centers and family day care homes.

The area south of Chicago served by Aunt Martha's includes both suburban and rural towns, some comprised of ethnically homogeneous, predominantly middle-class communities, others characterized by a greater socioeconomic mix, including less affluent, working-class, and poor neighborhoods. During the demonstration period, the program was located in a town that had a high unemployment rate and a substantial black minority population; the town and its surrounding area have experienced an out-migration of manufacturing employers.

During the period under study, the extent of Aunt Martha's catchment area and its suburban and rural character required staff to develop a diversified recruitment strategy to draw young mothers to the program. Staff made presentations at community agencies, hospitals, churches, schools, and a local chamber of commerce. Meetings were held with local welfare office staff, the state's Department of Rehabilitation Services, and the Job Corps. Public service announcements appeared on local radio and cable television stations, advertisements were placed in community newspapers, and flyers were posted in social service offices and local businesses.

The core New Chance staff at this site, a full-time coordinator and case manager, were supplemented by the job developer and instructors from other programs within the employment and training unit at Aunt Martha's, and from within other units in the agency. These staff taught the education, employability development, parenting, health, and family planning components.

Aunt Martha's New Chance program did not offer work internships, but several participants who attained GEDs and completed the program's Phase I activities enrolled in skills training classes at area providers. Few training courses were available for participants who had not passed the GED exam, and in general there were relatively few skills training providers within the area. The majority of participants who entered skills training classes enrolled in clerical and nurse's aide courses at the Advanced Technical Training Center, a major training provider for the area. Aunt Martha's' job developer assisted participants in finding jobs.

The women enrolled in New Chance experienced several serious problems, such as unstable housing arrangements and domestic violence, that prevented regular attendance at the program. Aunt Martha's attempted to address these problems through more intensive counseling and linkages with organizations that aid battered women and provide emergency housing. The development of an incentive program of monetary and non-monetary rewards for good attendance and program achievements was also part of the program's strategy to improve participants' attendance.

Aunt Martha's has developed a special linkage with Project Advance and Project Chance, the two JOBS programs operated by the Illinois Department of Public Aid (IDPA). Both projects served as referral sources for New Chance and, additionally, as sources of funding for child care and transportation for New Chance participants co-enrolled in either project. The New Chance program also received grants from Project Chance for education and training services for participants who qualified for co-enrollment in that project.

Despite state budget cuts in early 1991, IDPA renewed its commitment to fund New Chance. Aunt Martha's also obtained continued funding from other sources, including the local JTPA program – a provider of employment-related services and child care for JTPA-eligible participants – and the state's Board of Education, all of which ensured the continuation of services at the site, although the site has not been able to replace special demonstration-related funds that ended at the conclusion of the demonstration's operational phase in December 1992.

Since the time period under study in this report, the New Chance program has introduced a substance abuse prevention workshop to help address issues participants face in coping with drugs and alcohol abuse in their environments, particularly among their partners and other significant people in their lives. However, development of new services has been constrained by a persistent funding gap, although the site has continued to work on replacing the demonstration-related funds that it received through 1992, particularly with the aim of expanding case management capacity for New Chance participants. To close the funding gap, the program has operated since 1993 with reduced staffing. Funding constraints experienced by the New Chance program and other factors have prompted a recent decision at Aunt Martha's to integrate all participants in the agency's employment and training programs – male and female, parenting and non-parenting – into classes that follow the New Chance model.

Del Rey Center
Sweetwater Union High School District
Chula Vista, California

Sweetwater Union High School District's New Chance program was located at the Del Rey Center, which was formed from the merger of an alternative high school and an adult school in 1986. Adult education, occupational skills training, and counseling services are co-housed with a computer-assisted learning remediation center and Regional Occupational Center programs. (Regional Occupational Centers are funded by the state of California to provide education and occupational skills training programs, mainly to high school-age youth.) The Del Rey Center provides comprehensive education, occupational skills training, and child care services to in-school youth, high school graduates, and dropouts who are at risk of early pregnancy or who are already pregnant or parenting.

Two newly constructed buildings housed the New Chance classrooms, counseling and administrative offices, and an infant day care center. The Del Rey Center's Director of Vocational Education became the full-time director of the program. Part-time instructors were hired for adult basic education/GED, and workshops in LSO/parenting, employability development, and health/family planning. Full-time staff were hired for the positions of case manager and clerk-typist. The majority of the New Chance participants attended occupational skills training classes at local community colleges; others were enrolled in on-site training programs or in JTPA programs. The Del Rey Center provided free breakfasts and lunches for New Chance participants through WIC and other programs. In addition, the director gathered donations of goods and services for New Chance and for use as incentives for participants (e.g., home furnishings).

To facilitate implementation of the New Chance program, formal linkage agreements were made with several community service organizations. The primary agreement was with the San Diego County Greater Avenues for Independence (GAIN) Program. GAIN, a statewide program, is administered through the California Department of Social Services; with the passage of the Family Support Act in 1988, it became the state's federally mandated JOBS program. GAIN provides assessment, education, employability development, and vocational skills training to AFDC recipients, and funding for child care, transportation, and some ancillary expenses (e.g., textbooks, equipment, and uniforms) while participants are in the program; in addition, child care services and Medicaid coverage are extended for a year after participants begin work and discontinue welfare receipt. San Diego County GAIN identified AFDC recipients who met New Chance's eligibility criteria and mailed them material provided by the New Chance program. Recipients who co-enrolled in New Chance and GAIN were eligible for GAIN services.

The site's notable recruitment success was due in large part to the support of the GAIN program, but it also reflects the time and effort staff invested in maintaining the interest of potential applicants who had to delay participation – sometimes for several months – until a new enrollment cycle begins. Facing severe budget cuts in 1992, the Sweetwater Union High School District discontinued New Chance operations, and the San Diego County GAIN program continued to provide case management and services for those young women still enrolled.

Technical Education Center
Community College of Denver
Denver, Colorado

The Technical Education Center (TEC) is a branch of the Community College of Denver located just north of the Denver city limits. TEC has offered adult education, occupational skills training, and job search and job placement services to disadvantaged men and women since it was founded in 1983. The programs and services at TEC are individualized, with an emphasis on computer-assisted instruction. TEC offers four occupational skills training options: information processing (including word-processing), accounting, machine tool operating, and welding.

All New Chance services, except for health care and some child care, were provided on the TEC campus during the period under study. New Chance students enrolled in basic skills instruction and GED-preparation classes with other TEC students. New Chance participants also attended Living Competencies, a one-semester course exclusively for them, which encompassed parenting and child development instruction, family planning and health education, employability development, and the LSO curriculum. This course is a strong example of the integration of several different New Chance subject areas, as emphasized in the program's guidelines. TEC has incorporated the Living Competencies course into all of its core training options and has opened participation in it to any parent enrolled at TEC.

TEC was selected as a New Chance site because of its demonstrated success in helping disadvantaged people receive a GED and obtain occupational skills. Living Competencies was developed to include the health and personal development components of the New Chance model, as well as to strengthen the employability development component. The implementation of New Chance also led TEC to open an on-site developmental child care center in January 1990 that can accommodate 60 infants and toddlers. For children age 3 and older, New Chance used the Adams County Head Start program for day care, located near the campus.

The core New Chance staff at TEC includes a program manager/case manager and a Living Competencies instructor. TEC staff teach the GED-preparation and occupational skills training courses on campus, and guest speakers from local health clinics supplement the health education instruction.

The TEC New Chance program accepts residents from Adams and Denver counties. Before the program began, TEC already had a strong linkage for referrals with the Adams County Department of Human Development's JTPA program. Since the implementation of New Chance, the site has worked to develop a similar linkage with the Denver County Department of Social Services. Despite these efforts, recruitment has been the main challenge faced by TEC New Chance staff. Much of the eligible Adams County population is scattered in small towns throughout the county, and transportation is often difficult unless applicants have access to a car. In Denver County, transportation problems and competition from more established programs in the Denver metropolitan area have made recruitment of this population difficult. Among those enrolled in New Chance, however, attendance and retention rates have been high, due in large part to the staff's efforts to incorporate diverse learning strategies.

New Chance participants in Adams and Denver counties qualify for JOBS funding for child care and transportation support services, although the amount of child care funds continues to be inadequate and does restrict the number of teens who can be served. Also, JTPA funding has been used to pay for basic skills and occupational skills training, and for Living Competencies. Adams County's JTPA program recently received a grant to subsidize tuition at TEC for young mothers, and TEC itself received funding for the same purpose from the Colorado Community College and Occupational Education System.

In an effort to bring the New Chance services to more eligible teens who could not travel to TEC-North, the program was replicated in 1993 in another TEC campus. The Community College of Denver received a grant from JTPA to create a center in western Denver County (TEC-West) to serve 200 youth, including teen parents. The multi-year funding from JTPA, combined with grants from several other agencies and foundations, will provide education and training services for the youth. An on-site child care center, similar to that at TEC-North, will allow teen parents to participate in New Chance services, including the Living Competencies course.

As noted above, funding for child care remains a critical obstacle for serving more teen parents in both locations. While the Community College has been able to secure education, training, and transportation services for teens, inadequate child care funds prevents either location from reaching maximum enrollment.

Adams and Denver counties, and the Community College, remain committed to the New Chance model and hope to expand the reach of services to other parts of the counties as well. The development of proposals to foundations, state and federal agencies, and local organizations is an ongoing effort to both fill program funding gaps and attempt to expand the number of teen parents to be served.

Since the conclusion of the evaluation period, the New Chance program has continued to educate and support young mothers aged 16 to 21. This is accomplished through enrollment in three collegiate classes, worth a total of 9 credits, in parenting, child development, and family health. An additional benefit provided for each student is the assignment of a case manager, who acts as a liaison with supporting agencies and provides education and career counseling.

The goal of New Chance remains to help young parents develop skills to become educated parents and learn self-development in the areas of self-esteem, decision-making, communication, etc. These young parents also learn about domestic violence, relationships, furthering their education, culture awareness, child abuse, addictions, and health issues, to name a few. The learning environment takes place in a small group setting that provides openness, confidentiality, and camaraderie. Field trips are scheduled to such places as museums, zoo, parks, etc., where young parents learn to incorporate leisure activities into their lives. Additionally, a required lab activity is held once a week, during which the young parents bring their children and engage in play activities as an interactive process. They are also required to do two hours of volunteer work per week in the child care center, to gain further experience understanding children and their developmental levels.

Upon completion, the students are presented with a certificate of completion and a personal letter of encouragement from the instructor.

The class is open entry and tuition is provided through a federal grant. At present, the TEC-New Chance program has financial provisions for 24 students per year but hopes to expand that in the near future.

Development Centers, Inc.
Detroit, Michigan

Development Centers, Inc. (DCI), was formed in 1983 from the dissolution and reorganization of two highly respected and long-standing community mental health centers in Wayne County. DCI provides mental health services – including prevention, diagnosis, treatment, rehabilitation, and maintenance – and education services to children, adolescents, and adults residing in northwest Detroit and Redford Township.

DCI mounted a comprehensive support services program for high school-age parents in 1984. The program's on-site GED instruction, parenting education, developmental child care, individual and group counseling, and mental health services were important when DCI was assessed as a potential New Chance site. Although health education and services existed, they needed to be expanded to conform to the New Chance model. In addition, case management and employability development had to be added. The hiring of two case managers, a job developer, and a health educator, as well as linkages with local health care agencies, helped to address these issues.

Job-readiness training, referral to occupational skills training, and job placement assistance were also available before the implementation of New Chance; however, they were not core components of every participant's program experience. New Chance required DCI to move beyond its original concentration on education, parenting, and personal development to embrace economic self-sufficiency as a central objective for each participant. This shift in goals was accomplished through the efforts of DCI's executive staff, the New Chance project director and staff, and a local New Chance advisory group.

Shortly after being selected as a New Chance site, DCI moved from a neighborhood with high rates of teen births and welfare receipt to its present location. The move prompted concern because the New Chance program was no longer close to where those most likely to apply lived. In response, DCI staff raised public and private funds for two vans to transport participants and their children to and from the site.

During the period under study, the project director and two case managers (all full-time staff of DCI) made up the core New Chance team. Their efforts were supported and complemented by staff from other DCI programs and outside agencies. DCI's parenting instructor and health educator worked part-time for New Chance. The Detroit public schools co-located two instructors at DCI to deliver individualized GED preparation and adult basic education. Classroom instruction was supplemented with practice activities in the Apple computer lab. Health education and services, work internships, vocational training, guest speakers, and field trips were available as a result of extensive outreach to community-based and public agencies.

A local advisory group guided and supported New Chance. The group helped persuade public and community-based agencies to provide services and other resources to New Chance, and funding development was an explicit part of its charge. The addition to the advisory group of the director of a Wayne County welfare office paved the way for referrals and other assistance from two additional county welfare offices. These referrals helped DCI to enroll 175 women. The assistance of this official has also enabled DCI to secure funds from Michigan's JOBS program.

Another important feature of DCI is its child development program. The child care center is licensed and has a full-time director. The child development program is a joint venture involving the child care, infant mental health, and parenting instruction staff, who carefully monitor parent-child interaction, intervene when necessary, and deliver consistent messages about appropriate parenting practices.

Poor attendance and attrition are the major operational challenges confronted by DCI. Staff believe that many of those with poor attendance applied to the program, only because they feared that welfare sanctions would be imposed if they did not enroll in an education or training program, as required by law. Yet those young women who are more positively motivated to enroll also face obstacles to regular participation that include illness, housing problems, personal and family crises, and repeat pregnancies. To overcome these obstacles, staff increased counseling, classroom instruction, and referrals to outside agencies.

The prospects for institutionalizing New Chance at DCI were unclear because of the state's fiscal crisis. There were a number of factors working in DCI's favor, however. DCI's executive leadership is solidly behind New Chance. The program received support from its Congressional representative, staff from the state's human services agencies, and local political and community leaders. A variety of private funding sources have been approached for support to continue New Chance operations. Most importantly, DCI was recently awarded one of four state contracts to provide services to teen parents.

Since the time period under study in this report, the site has continued to refine its services. It is now possible for enrollees to remain in Phase 1 for up to 12 months to give those who need it more time to develop their skills. DCI has also strengthened its support system for participants after they leave the program by providing a case manger to do follow-up. The parenting component has also been enhanced, with daily parent/child sessions at lunchtime.

Mid-Manhattan Adult Learning Center Office of Adult and Continuing Education New York City Board of Education New York (Harlem), New York

Located in the Harlem area of New York City, the Mid-Manhattan Adult Learning Center (MMALC) is one of several adult schools operated by the New York City Board of Education's Office of Adult and Continuing Education. MMALC's New Chance program built on and integrated a sequence of services available at the school: GED, life management, and pre-vocational courses, and a wide variety of vocational training offerings. MMALC's participation in the New Chance Demonstration was co-sponsored by the New York State Department of Social Services. New Chance participants were co-enrolled in BEGIN, New York City's AFDC employment program operated by the Human Resources Administration (HRA), which funded transportation expenses and, when needed, off-site child care provided through the Agency for Child Development.

MMALC is well-known for its intensive educational and vocational preparation. The school's reputation in these areas was a key factor in its selection as a demonstration site. The New Chance education, occupational skills training, and adult survival skills activities drew most heavily on MMALC's areas of expertise and experience, and were among the New Chance program's strongest elements. Most of the services required by New Chance were already available at MMALC, and during the period under study, several MMALC staff were brought into the program on a full- or part-time basis. In some instances, New Chance participants were in classes specifically designed for them, but they also attended classes with other MMALC enrollees.

For example, during the period under study, MMALC developed a separate education class for the program. MMALC's adult basic education and GED instruction were individualized and computer-assisted, but group instruction was also provided as a means of motivating students and helping them to develop reasoning and communication skills. Life management classes have been a part of the core curriculum at MMALC for many years. The instructor for the New Chance life skills class used class discussions, audio-visual materials, field trips, and guest speakers to address the topics required in the New Chance health and personal development components, including legal and consumer issues, personal and family health matters, and citizenship and civic responsibilities. The content of MMALC's life skills class complemented New Chance's class in decision-making skills, which was added when New Chance was implemented.

Some services did have to be added or expanded to conform to the New Chance model: family planning education and case management services were added, and existing parenting, child care, and health education services were expanded. The Board of Education's Learning Through Young Family Education (LYFE) program renovated and equipped two rooms at MMALC for use as an infant and toddler day care center. LYFE also provided licensed, trained staff for the center. The effort to add or expand services in the health and personal development components also drew on other agencies in the community: Family Dynamics Inc. conducted parenting classes, Planned Parenthood provided family planning workshops, and Harlem Hospital, through its community outreach effort, supplied guest speakers and a leader for health education workshops.

Phase II of New Chance included unpaid internships at government and community agencies, as well as paid work experience obtained through JTPA Summer Youth Employment Program positions. Case managers were responsible for helping GED recipients make the transition into skills training, assisting them as they decided what type of training they wanted to pursue and where they wanted to enroll. Only rarely were participants encouraged to enroll in skills training courses prior to passing the GED exam. MMALC was the principal provider for skills training courses for New Chance participants, although some who attained their GED through the program opted to enroll at community colleges. New Chance participants selecting occupational skills training programs at MMALC were placed in regular classes with other MMALC training students.

During the first year of program operations, developing long-term linkage arrangements with outside agencies to provide New Chance's family planning and parenting components was challenging for the program. Staff changes at linkage agencies resulted in interruptions in service delivery and, in some instances, necessitated changes in linkage arrangements. Through the persistent efforts of program management, new staff or linkage agencies were found and integrated into the program. Staff turnover among MMALC staff teaching the New Chance GED class was another management concern during the period covered by this report.

Recruitment was also a challenge during the early period of program implementation. However, MMALC's strong relationships with other agencies in the community and with HRA enabled it to marshal support for New Chance recruitment efforts and to meet the enrollment target, despite the fact that recruitment got off to a slow start and began later than planned because completion of the on-site infant child care facility was delayed.

Since the period under study in this report, there has been a greater emphasis on integrating New Chance participants into regular classes at MMALC; New Chance enrollees have fewer separate classes and more classes in which they are integrated with other MMALC students. An additional change since the period under study has been increased use of MMALC's clerical and health occupation pre-vocational classes, which combine education classes and introductory, hands-on vocational training activities related to the career focus. New Chance participants scoring below 9.0 on the TABE reading test have been placed in one of MMALC's two pre-vocational courses; enrollees scoring 9.0 and above have been placed in the school's GED preparation class. Both classes have had access to MMALC's new parenting center for weekly parent/child literacy training and other interactive activities.

MMALC's New Chance program is continuing beyond the demonstration period, but with reduced staffing and enrollment. A single staff person is both the coordinator for the program and the sole case manager.

Youth and Family Center
Inglewood, California

The Youth and Family Center (YFC), a nonprofit organization established in 1979, is located in downtown Inglewood in Los Angeles County. YFC also provides services at three local high schools, but services for New Chance participants (with the exception of occupational skills training and child care) were brought together under one roof at YFC's main site. Before the start of New Chance, YFC provided counseling and family planning services to pregnant and parenting teens 18 years old and younger. In 1990, its programs served more than 200 young women and 50 teen fathers.

Although several of YFC's services needed to be strengthened for New Chance, the organization brought to the demonstration a dedicated, high-quality staff; a history of operating highly regarded, comprehensive programs; experience working with the male partners of young mothers; individual counseling and guidance services; drug and alcohol rehabilitation; and a strong AIDS prevention program.

In implementing New Chance, an agreement was reached with the Los Angeles County Department of Public Social Services to design shortened intake procedures for enrolling New Chance participants from the local Greater Avenues for Independence (GAIN) Program, California's JOBS initiative. GAIN provides assessment, education, employability development, and vocational skills training to AFDC recipients. Co-enrollment of New Chance participants in GAIN ensured that the California Department of Social Services would provide funding for support services such as child care, transportation, and some education- and training-related costs while the young women were in New Chance.

Because the Los Angeles GAIN program did not target teenage mothers for service during the time the research study group was recruited, neither the welfare agency nor GAIN referred eligible young women to New Chance. YFC staff were, however, permitted to recruit in person at the county's Income Maintenance office. This meant that recruitment occupied a great deal of staff time and attention.

Another linkage was with Inglewood's JTPA agency, which provided funding for the purchase of the Comprehensive Competencies Program (CCP), a widely used computerized literacy and math training system. CCP was installed at the site in late February 1991, and all New Chance staff received 40 hours of training in its operations. Inglewood School District instructors taught the GED, employability development, and LSO classes. Also, early in 1990, YFC was successful in reaching an agreement with the local Regional Occupational Center (a state-funded provider of education and occupational skills training programs) to provide on-site word-processing training to New Chance participants. Other participants received occupational skills training in JTPA or community college programs. In addition, 10 family day care providers were recruited to serve participants' children. YFC staff met with these child care providers regularly, both offering them support and training and receiving feedback on the children.

Building an integrated New Chance program in the Inglewood area presented special challenges to YFC staff: They had to negotiate with the school district for instructional staff; with the local social services agency to be allowed to recruit at the AFDC office; with the GAIN program for priority treatment of New Chance enrollees; and with myriad training providers, each with its own geographical limitation or operational complexities. YFC met these challenges, however, and was successful in putting together a high-quality New Chance program. However, by 1994, continuing difficulties with these bureaucracies, coupled with declining funding, overwhelmed the program, and YFC ceased program operations during the first quarter of the year.

The Bridge
Family Health Services, Inc.
Jacksonville, Florida

The Bridge is a 12-year-old multi-service center for children and youth operated by The Bridge of Northeast Florida, formerly known as Family Health Services. Family Health Services was established in 1972 as a coordinating agency for family planning services for low-income women and teenagers. The Bridge was created as a model to foster youth and family development. While health-related services remain a key focus, The Bridge's mission is to help low-income youth stay in school, prevent early pregnancy, and avoid juvenile delinquency, substance abuse, and abuse and neglect of children. The Bridge serves more than 2,000 children (ages 0 to 2) and youth between the ages of 6 and 19 through its clinic, which provides reproductive and general health services, an after-school tutoring program, short-term counseling, and case management services.

The New Chance program at The Bridge was co-sponsored by The Ounce of Prevention Fund of Florida, the local JTPA agency, the Private Industry Council (PIC) of Jacksonville, and Florida's Department of Health and Rehabilitation Services (HRS). Program services were offered in conjunction with the Florida Community College at Jacksonville (FCCJ). These co-sponsoring organizations have a history of collaboration in delivering employment services to the city's disadvantaged populations. The program is located at The Bridge of Northeast Florida's offices in an inner-city, predominantly black, residential neighborhood.

Broad experience in providing health, family planning, and parenting services to disadvantaged young mothers was one of the main reasons for selecting The Bridge to be part of the New Chance Demonstration, as well as the history of collaboration among the agencies expected to help deliver component activities in areas in which The Bridge had less experience. While several types of activities and services were available to young mothers by appointment or on a drop-in basis, before New Chance, The Bridge had not offered a comprehensive, daily program for this population. Mounting New Chance at this site involved adding new services, expanding the scope of existing activities, and shifting the agency's orientation to operating a full-time, comprehensive program.

The New Chance program built on The Bridge's strong parenting, family planning, and health education services. Other facilities and Bridge staff also became part of New Chance. The Bridge's on-site clinic provided prenatal and well-baby care, family planning counseling, and treatment for sexually transmitted diseases; other health services were provided at a nearby hospital.

While new staff were hired to provide case management, a different strategy was used to mount most of the additional services called for in the New Chance model. The Bridge developed a linkage with FCCJ to provide instructors for on-site adult basic education and GED classes. New Chance's employability development activities, another key component of the first phase of the program, were offered on-site through an arrangement with the local JTPA PIC, which assigned a staff person to the New Chance program.

The program's Phase II activities were also implemented with significant help from the collaborating agencies. A PIC staff person was instrumental in placing New Chance participants in JTPA paid work experience positions developed by and funded through the PIC. The program also used jobs available through JTPA's Summer Youth Employment Program as a way of offering paid work internships to participants. (Unpaid work internships were not offered during the period under study.) FCCJ offers a wide range of vocational courses at its nearby downtown campus and was the primary occupational skills training provider for New Chance participants. Tuition was mostly funded by federally provided Pell Grants, but tuition waivers were available to some participants through HRS. Job placement was handled by the same PIC staff person who led the program's employability development classes.

Child care, funded by HRS and the PIC, was available at nearby centers; one of the centers was for a time able to give priority to New Chance participants. To qualify for HRS-funded support services, including child care and transportation, New Chance participants had to be co-enrolled in Project Independence, Florida's JOBS program.

The Bridge's linkage arrangements and the services delivered through them were exceptionally consistent. The agency's greatest challenge in implementing New Chance was to develop the structure for an ongoing, intensive program for a hard-to-serve population. New Chance brought with it a host of management issues – including creating and implementing incentive structures, rules, and attendance standards, and developing staff consensus on expectations for participants and appropriate responses to their behavior – that are not typically encountered when services are by appointment or of limited duration, as had been usual at The Bridge before New Chance. Irregular attendance, and its effects on program services and participants' progress, was an issue with which the site had little experience. As one way to address this problem, staff implemented an incentive program in which participants who met attendance requirements could earn points exchangeable for household items that could not be purchased with food stamps.

Since the time period under study in this report, the site has instituted unpaid work internships through a component it calls the Real-World Internships Project. Corporations and United Way agencies have responded to the New Chance program's effort to expand the number of internships it can offer through provision of short-term unpaid internships that nonetheless have provided opportunities for participants to explore jobs and careers and practice and reinforce skills learned in employability development classes. Other changes have included integrating family planning into all other facets of the program and developing a bimonthly class that allows for a focus on hands-on parenting instruction, despite not having an on-site child care center. Participants have brought their children with them to the program on Fridays, which have been — with the exception of the parent interaction class — reserved for staff planning activities and meetings. The parenting instructor has developed activities for the parents and children to do together while she observes and facilitates their interactions. In addition to receiving feedback from the instructor, participants also have had an opportunity to observe the instructor model ways of encouraging positive development in children.

The prospects for continuing New Chance at this site appear good. The linkages to FCCJ and the PIC for in-kind support for education and employment activities, and to the PIC, HRS, and Project Independence for child care funding are continuing, though child care funding available through Project Independence has tended to be oversubscribed since 1993, with few funds available for New Chance. In addition, a new ongoing funding arrangement has been developed with the Ounce of Prevention Fund, and the program has had a contract with the PIC for JTPA funding since 1992. Program staff are also pursuing additional sources of ongoing funding.

The Family Care Center
Lexington, Kentucky

The Family Care Center (FCC), a program of the Lexington-Fayette Urban County Government's Department of Social Services' Division of Family Services, had been designing a program to help AFDC recipients achieve self-sufficiency just as the New Chance Demonstration was evolving. When FCC opened in 1989, it replaced the Early Child Care Center, which had provided pediatric health care and social services to at-risk children. Because the Early Child Care Center had never operated a program specifically for teen parents, a New Chance program at this site was not able to build on an existing infrastructure. However, FCC was a good candidate for the demonstration because plans were already under way there to build a multi-service center and operate a comprehensive program for AFDC recipients. Also, the commitment of FCC's director, staff from collaborating agencies, and the Urban County Government, and the support shown by the Cabinet for Human Resources (CHR) – Kentucky's state welfare agency – provided a compelling rationale for including FCC in the New Chance Demonstration.

FCC provides the comprehensive, multi-generational services required by the New Chance model. Including New Chance participants, FCC serves approximately 200 children and more than 100 teenage parents and AFDC recipients annually. The facility includes child development classrooms, adult classrooms, and observation rooms, as well as a cafeteria, playroom, parent resource center, vocational assessment laboratory, computer laboratory, study area, library, and exercise room. The University of Kentucky staffs the comprehensive dental, preventive health (pediatric and adolescent), and medical care facilities located at FCC. An adolescent clinic is staffed by a University of Kentucky medical team one day each week.

Once FCC was selected to be a New Chance site, its director began building the program, and four case managers were hired. Contracts with the Fayette County Public School System, the local JTPA program, and other public agencies have allowed staff to be co-located at FCC to deliver education and employment-related instruction. The teachers used the New Chance guidelines to design their own curricula and instructional strategies, and the GED and adult basic education instructors mixed group and individualized instruction with computer-assisted instruction to create an innovative learning environment. FCC's parenting education, health services, and child care directors assumed responsibility for those aspects of New Chance, and FCC's child psychologist also provided support. Planned Parenthood offered family planning education and services to New Chance participants. While vocational skills training was not available on-site, participants had access to education and training programs offered by Lexington Community College, JTPA-funded agencies, and other training providers. Many local employers agreed to provide job-shadowing opportunities and work internships to New Chance participants.

New Chance continues to receive encouragement and support from CHR, the Lexington-Fayette Urban County Government, local public and private agencies, and a volunteer board, which helped the program gain widespread community support and helped staff fulfill the implementation and enrollment objectives and to develop a strategy for rewarding attendance and achievement.

FCC staff have actively pursued solutions to implementation problems at the site. According to staff, participants' feelings of powerlessness and their lack of self-esteem and basic skills account for the repeat pregnancies seen at FCC; physical abuse and homelessness have also plagued many of the participants. Methods to overcome these obstacles to program success include increased personal counseling, referral to outside services, reinforcement of the skills taught in the Life Skills and Opportunities curriculum, positive peer interaction, and additional classroom instruction.

While teen mothers are still not a target group under JOBS in Fayette County, welfare staff have referred them to New Chance. FCC was selected as one of the four New Chance sites for a 17-month federal demonstration grant to enhance services and encourage a stronger relationship with the JOBS program. As a result, a JOBS caseworker has been assigned to FCC for a portion of each week to expedite any changes in benefits and support services for JOBS-eligible teen mothers.

As a recipient of the national grant, FCC has begun to enjoy more exposure and has ably promoted the organization to state and local funding agencies. FCC continues to receive financial support from CHR to supplement the national grant and is hopeful that the program will receive a long-term financial commitment of state funds.

FCC continues to seek other funding provided by national and local foundations as an innovative program aimed at providing a comprehensive service delivery network for teen mothers. Such demonstrations are ideal for FCC which, except for vocational training, houses education and life skills classes along with medical and child care services. These resources, along with the support FCC receives from CHR and the Urban County Government, make the long-term outlook for this New Chance program promising.

RESOURCE, Inc.
Minneapolis, Minnesota

RESOURCE, Inc. is a not-for-profit organization offering vocational rehabilitation, employment, chemical dependency, and mental health services to low-income individuals in Hennepin County and the city of Minneapolis. The New Chance program, which serves only women who reside in the city, is part of RESOURCE, Inc.'s Employment Action Center (EAC) located at the Sabathani Center, a multi-service community agency in south-central Minneapolis.

The strength of EAC is its employment and training programs. By hiring experienced staff it has been able to implement the New Chance health and personal development components and basic education classes. The program has also created linkages to local organizations such as the Minneapolis School District for GED preparation and MELD for parenting instruction and staff training.

Integration of the messages and skills of the various New Chance components has been a special achievement of the Minneapolis program. With the donation of Apple computers, staff have set up a computer learning center used to enhance GED-preparation instruction and the health, personal development, and employability development components. Instructors wrote their own New Chance workbook that included group and individual exercises and computer activities related to women's history, budgeting, life skills, reading and writing improvement, and career choices.

The GED-preparation class has improved

markedly with the addition of computer-assisted instruction and the hiring of a full-time GED instructor, and a high percentage of participants have passed the GED test. The site also has a strong relationship with STRIDE (Success Through Reaching Individual Development and Employment), Minnesota's JOBS program. STRIDE mandates participation in education programs for young mothers on welfare without a high school diploma or GED, and enrolling in New Chance is an option for fulfilling this requirement. The local STRIDE office helped New Chance staff to recruit for the program by providing lists of potentially eligible welfare recipients. New Chance staff also present information on the program at STRIDE orientation meetings and follow-up to any women who express an interest. In addition, STRIDE pays the training and child care costs associated with participation in New Chance, as well as a portion of the case managers' salaries.

In response to early attendance and retention challenges, the site developed a number of interesting work internships with the city, a department store, and a hotel that have drawn participants into training and kept them in the program.

The Minnesota Department of Human Services provided the initial state grant to RESOURCE, Inc. for the demonstration. RESOURCE, Inc. has successfully negotiated with the department to provide the program with four more years of funding, which will allow the site to continue enrolling young women in New Chance. In addition, RESOURCE, Inc. was one of only three New Chance sites selected by the Administration for Children and Families of the U.S. Department of Health and Human Services to participate in a national demonstration of the effects of offering enhanced case management and home visits in conjunction with New Chance services.

RESOURCE, Inc. has been able to strengthen and build the New Chance program since the period under study in this report. Staff have added an on-site psychologist and a transition/job developer, and have supplemented program services with college classes offered on site, peer parenting counselors, a mentoring program, a weekly job club, early childhood family education, more home visits by case managers, and a Job Service terminal on site. The most recent addition has been a young father's program.

Lutheran Settlement House Women's Program
Philadelphia, Pennsylvania

The Lutheran Social Mission Society (LSMS) is a nonsectarian, nonprofit community-based organization that has been devoted to meeting the needs of disadvantaged children, youth, and women, and also of the elderly. Since 1976, one of its divisions, the Lutheran Settlement House (LSH) Women's Program has provided adult basic education, vocational training, services for victims of domestic violence, and a senior day care program. Located in Fishtown, one of Philadelphia's inner-city neighborhoods, many services of the Women's Program are targeted to disadvantaged minority women and their families.

The New Chance program built on and operated as part of the existing Teen Parent Education/Employment Program (TPEEP), which the Women's Program had been operating since 1987. TPEEP enabled LSH to begin New Chance with quality education, parenting, and job-readiness services for adolescent parents already in place. With the addition of New Chance, however, the program's scope, duration, and size all changed: Staff expanded the program's focus on employability development, preparation for vocational training, health, family planning, and life skills; the duration of the program increased from 4 to 6 months to up to 18 months; and the program's capacity doubled.

During the period under study, services were provided by Women's Program staff who worked full-time with the New Chance program and through linkages with outside organizations that were enlisted to enhance curriculum and services in the areas of health, family planning, parenting, and life skills. These linkage organizations included (but were not limited to) Jewish Family Services, Planned Parenthood, the Penn State Cooperative Extension Service, and an AIDS education peer-counseling program, all of which conducted workshops for New Chance on an ongoing basis.

Throughout the demonstration, the TPEEP/New Chance program also benefited from other historical linkages with agencies in the community. The TPEEP/New Chance staff mounted a successful recruitment campaign that enabled the program to reach its enrollment goal in less than a year through aggressive outreach and strong partnerships with local county assistance offices and JTPA staff. On several occasions, the local welfare department sent out recruitment letters to teen parents on its rolls. The good working relationships with caseworkers in local county assistance offices both supported the active participation of the teens enrolled in New Chance and generated new referrals. The site's relationship with the local JTPA agency, the Philadelphia Private Industry Council (PIC) – the TPEEP/New Chance program's largest funder – was vital. The PIC was instrumental in helping to resolve barriers faced by New Chance participants in making the transition to vocational training programs and also generated referrals for the program.

During the first year of the demonstration period, the small team of core TPEEP/New Chance staff managed to implement and operate all the components of New Chance, and to keep the participants in the program despite recurring fiscal constraints, problems with the physical plant, and management changes in the parent organization. The staff were also challenged by the multiple difficulties facing many of the New Chance women, including physical or emotional abuse by family members or partners, drug and alcohol abuse in these young women's families, and unstable living arrangements.

The program staff demonstrated facility in integrating Phase I lesson content across New Chance subject areas and in making the information taught relevant to the young women's lives. The constant support and counseling provided by the case managers were a hallmark of the program, and reinforced how participants could apply the life skills learned in New Chance to their day-to-day lives.

For New Chance, the program hired an employment specialist who coordinated all aspects of the employment preparation and job placement. The specialist taught the program's employability development classes, arranged for internships and enrollment in skills training, and helped participants get jobs. Philadelphia had an advantage in that skills training providers were more likely to accept enrollees without a GED than was typical of many other New Chance communities; the specialist's efforts to move participants into training were hindered, however, by reluctance among many JTPA providers to risk serving teens and their lack of understanding about the New Chance program's ability to help support and monitor New Chance enrollees while they attended training classes. The program's persistent appeals to the PIC eventually led to the participants' improved access to courses and better communication between the New Chance program staff and the training providers.

Since the time period under study in this report, in response to contractual benchmarks in its contract with the Single Point of Contact (SPOC) Program – part of Pennsylvania's JOBS program – the site has incorporated a greater emphasis on helping participants make the transition into skills training and has significantly increased the proportion of enrollees who participate in vocational courses, exceeding the goal set in the SPOC contract. There is less emphasis on GED attainment, and the goal of the program's education classes is seen more as the achievement of educational benchmarks and enhancements (including, where appropriate, the GED) needed to enter skills training. The program has also developed an ongoing group counseling workshop led by a psychologist. The weekly sessions focus on mental health issues of concern to participants.

The TPEEP/New Chance program was

supported by funds from the SPOC Program, supplemented by contributions from private and corporate foundations. All New Chance participants receiving AFDC were enrolled in SPOC. The program has continued to receive SPOC funding beyond the operational phase of the demonstration and has been able to maintain both the additional programmatic components and the enrollment levels achieved during the demonstration.

Pittsburgh in Partnership with Parents
Hill House Association
Pittsburgh, Pennsylvania

Hill House Association (HHA) is a multi-service, community-based agency that has served residents of Pittsburgh's Hill district since 1964. New Chance is a component of Pittsburgh in Partnership with Parents (PPP), a program started in 1986 to offer educational and employment opportunities to young parents, both male and female. PPP operates under the management of HHA's executive director and is located on the agency's premises. Direction is also provided by an Advisory Committee composed of representatives from the city's public, private, philanthropic, and not-for-profit sectors. PPP's location in the heart of the Hill district, one of the city's oldest, historically black, inner-city neighborhoods accounts for the program's predominantly black enrollment.

As one of six agencies to participate in the national pilot phase of the New Chance program, HHA/PPP entered the demonstration with substantial operating experience. All of the model's components were in place at HHA/PPP at the beginning of the demonstration, and its challenge has been to refine implementation of activities and objectives. At the conclusion of the New Chance pilot, HHA/PPP revised the pilot phase's intensive, almost exclusive focus on education during the early months of an enrollee's participation in New Chance to permit a greater concurrent emphasis on employability and personal development activities. In addition, the employability development component was restructured to include an intensive career exploration phase following GED receipt. To support these changes, HHA/PPP developed its own curriculum guide for both education and employment-preparation activities.

During the demonstration period, the sequence of activities specifically aimed at preparing participants for employment was one of HHA/PPP's strongest program elements. While HHA/PPP's approach shared some of the characteristics of strategies used at other New Chance sites, there were important differences. Participation in employment-related activities began at program entry, with participation in introductory classes related to career exploration led by the training specialist and job-readiness classes led by the program's job developer. Participation in these activities continued as students progressed toward taking the GED test.

However, intensive examination of career possibilities, including work internship and job-shadowing opportunities, was scheduled during a multi-week program phase that follows GED receipt. Courses in the career exploration phase were largely the responsibility of the training specialist. At the completion of this phase, the specialist helped participants select a career area and a vocational training course, and was responsible for monitoring their progress. Following the completion of training, the job developer assisted participants to obtain employment.

While Pittsburgh enrollees faced many of the same problems as those at other sites, perhaps the greatest ongoing challenge to the site has been managerial: that of integrating staff from a variety of agencies. HHA/PPP may well represent the demonstration's strongest example of a program in which almost all services were delivered on-site but were also brokered. Only administrative, case management, and employability development services were provided by staff entirely on HHA/PPP's payroll; all other activities were conducted by full- or part-time staff from collaborating agencies under in-kind or contractual arrangements. To foster program cohesiveness, staff participated in an annual retreat held each summer to review the prior year's progress, successes, and disappointments. These retreats supplemented routine staff meetings held on a monthly basis.

Since the time period under study in this report, the Pittsburgh program has concentrated on enhancing the parenting component and the program's services to young fathers. For example, participants have planned social activities with games and activities for themselves and their children for major holidays throughout the year. Also, separate classes, including a life skills/decision-making curriculum oriented toward males and a Rites of Passage class that explores the cultural aspects of being a man in our society, have been developed for the program for fathers, who previously shared almost all classes with the young mothers.

Another change has been a greater emphasis on the JTPA youth competencies as an outcome of PPP's employability development classes. JTPA staff have been asked to help evaluate participants' attainment of the competencies at the end of the year in June through assessments of participants' class portfolios and their demonstrated skills. For example, JTPA staff have evaluated participants' interviewing skills through playing the role of employer in mock interviews. The involvement of outside reviewers leads program participants to take the assessments seriously and to be more rigorous in preparing for them.

HHA/PPP is a well-institutionalized program that enjoys wide support at the local, state, and national levels. The Single Point of Contact (SPOC) Program – part of Pennsylvania's JOBS initiative – is HHA/PPP's principal source of funding, and all New Chance participants who received AFDC were enrolled in it. The continuation of the program as implemented during the New Chance demonstration seems secure as most of the program's demonstration-related SPOC funding remains in place. The program is exploring potential sources for filling remaining funding gaps.

PIVOT-New Chance Program
Portland Public Schools
Portland, Oregon

The New Chance program in Portland is a joint effort of the Portland Public Schools and the Portland Job Corps. New Chance, which is known as PIVOT (Partners in Vocational Opportunities Training), is an outgrowth of the school district's Continuing Education for Girls (CEG) program. CEG, now know as Monroe, has been operating for 19 years as an alternative educational setting serving pregnant students, who usually return to their home schools at some point after the birth of their child. Monroe students tend to be younger than those enrolled in New Chance and usually are not high school dropouts. Monroe offers an accelerated high school curriculum and a GED-preparation curriculum, as well as parenting, health, and counseling services.

CEG formed a partnership with the Portland Job Corps to implement the full New Chance model. CEG provided education, health, and personal development services, and the Job Corps – with special federal funds – provided the employability development and occupational skills training services, stipends, and support services. Drawing on CEG and Job Corps personnel, PIVOT-New Chance developed one of the largest staffs in the demonstration. A special staff position was created to recruit and enroll New Chance participants, which allowed the site to increase its enrollment goal. In addition, the site was one of the first to recognize the range of participants' problems calling for outside intervention; accordingly, it arranged for the Oregon Health Services University to provide services, including mental health counseling, at the on-site health clinic and for treatment for substance abuse to be provided by maintaining a community drug and alcohol program.

There are several notable features of the PIVOT-New Chance services. The GED classes have been strengthened by the use of computer-assisted instruction, which gives the teacher more time to work individually with students, and pre-GED classes are provided through literacy labs. The program also offers a two-semester business skills training course at the site that includes word-processing, typing, "10-key" (use of an adding machine) and record keeping, business English, and telephone skills training. Advanced career training is offered through Portland Commercial College. In addition, on-site child care is available at the Head Start Infant and Toddler Center, and van transportation and bus passes are provided by the Job Corps. Enrichment courses in life skills are also provided.

The greatest challenge facing the collaboration in Portland has been fulfilling the different program requirements, streamlining them for administrative effectiveness, and recognizing the varied organizational philosophies of the agencies involved in implementing and operating New Chance. Development of a joint management structure has allowed input from the primary agencies involved, including MDRC, to ensure that each agency's goals and requirements are met. The program is a model collaboration program that utilizes the best curriculum -- MDRC, Job Corps, and Portland Pubic School offerings. Its structure and community linkages have been strategically designed to enhance service delivery and funding opportunities. In working with its collaborative partners, the challenge still remains one that strives for (1) shared vision, (2) shared mission, and (3) shared resources, the keys to its success.

Through the efforts of the Job Corps and Monroe staff, community awareness of the program has grown. For example, PIVOT Pals, a network of businesses, ensures regular donations of money, goods, and services to the program through such activities as sponsoring monthly awards luncheons for participants at local restaurants; providing work experience opportunities, telephone skills training, and job placements; and collecting gift certificates for use as program incentives. Students have also been featured in television news stories and newspaper articles.

Oregon's JOBS program, which began in October 1990, emphasizes enrolling young mothers on welfare in high school or GED programs. Consequently, Portland Public Schools has a contract with JOBS to identify young mothers who have not finished high school, assess their need for services, and facilitate enrollment in one of several district program options, including PIVOT-New Chance. The site also receives child care funding through the JOBS program. In addition, staff are involved in the local welfare office's planning committee for JOBS services to teens.

As one of 10 national programs funded through the U.S. Department of Health and Human Services, the program also receives state funding for New Chance, and Job Corps funding is secure through October 1995.

Teen Parent Program
The YWCA of Salem
Salem, Oregon

The YWCA of Salem is an affiliate of the national service organization and serves residents of Salem and the neighboring counties. The YWCA has operated a teen parent program for 27 years, providing such services as education, employment skills training, and child care. The Teen Parent Program, which included New Chance during the research project, moved to new facilities at the Oregon School for the Deaf in 1990. The building it currently occupies, a former dormitory, has classrooms, meeting rooms, offices, and a child care center.

When the YWCA was chosen for the demonstration, the education, parenting, health education, and counseling services were the backbone of its teen parent program. For New Chance, the site increased its emphasis on employability development and attainment of a GED. The YWCA already had linkages with the Salem/Keizer (24J) School District, Chemeketa Community College (CCC), and the Marion County Public Health and Mental Health departments, which all became service providers for New Chance participants as well. The

YWCA also works with the state's Executive Department as part of a business/school partnership program in which Executive Department staff volunteered to become mentors for New Chance participants. The department also accepted New Chance students into its clerical training courses on a space-available basis, and members hosted holiday dinners and donated clothing and toys to the participants.

Most New Chance staff were employed by the YWCA. The GED instructor was provided by CCC. The YWCA was one of five New Chance sites to have received a donation of Apple computers to develop a computer learning center, which was used for GED instruction and employability development activities. After participants completed the GED course, they could enroll at CCC for occupational skills training courses. Job placement assistance is provided at the Teen Parent Program by an employment teacher and through the Mid-Willamette Valley Jobs Council (the local JTPA agency).

The YWCA operates a child care center for infants and toddlers at the site, and New Chance participants received priority for its full-time services. Participants are also co-enrolled in the public school district, allowing them to receive free lunch and transportation services.

Relative to other New Chance programs, the Salem site has a small number of potentially eligible young women in the area. However, the staff's persistence in recruiting enabled the site to meet its enrollment goal by June 1991. Securing steady attendance and retaining enrollees were major challenges, however. Efforts were made to reenroll participants who dropped out of New Chance because of health and family problems. Another issue was turnover among program personnel at both the managerial and instructional levels.

JOBS was implemented in the Salem area in October 1991. The local welfare office has contracted with Chemeketa Community College to provide services to welfare clients, which has made the YWCA's linkage with CCC even more important. Site staff met with CCC and welfare office staff, which facilitated New Chance. In addition, the YWCA worked with CCC to help New Chance participants gain greater access to the occupational skills training and job search programs at the college.

Program staff are negotiating with the state to secure future funding for New Chance. The YWCA is also requesting support from several regional foundations.

Independence Adult Center
East Side Union High School District
San Jose, California

In 1988, the Independence Adult Center merged with the East Side Union High School District. With state funds, the district provides subsidized child development and child care services to low-income families and children at risk of neglect and abuse. It also operates preschool programs on seven high school campuses and – at a separate facility – the Family Learning Center, which provides child care and support services to in-school pregnant and parenting teens through age 17. The Family Learning Center enjoys a statewide reputation for excellence. Although implementing New Chance required the Independence Adult Center to start a new program, it had many services already in place on which to build, and staff were excited by the opportunity to expand existing services to serve an older population. The Independence Adult Center serves all adults who apply, many of whom meet New Chance eligibility criteria.

Approximately 6,000 youths drop out of school annually on San Jose's east side. In addition, a large proportion of the teen births in Santa Clara County are among residents of this area: In 1988, for example, there were 2,170 births among females between the ages of 11 and 19, accounting for 54 percent of all teen births in the county. The east side is also home to 80 percent of the county population eligible for Greater Avenues for Independence (GAIN), the state JOBS program.

Staffing New Chance was a major challenge during the period under study, given the strict hiring requirements imposed by the school district, but the program assembled a strong and cohesive staff. To provide all the services required by the New Chance model, the site also negotiated linkage agreements for occupational skills training with the San Jose Job Corps, the Central County Occupational Program, and the Center for Employment Training. Many participants attended local community colleges for training as well.

The most significant linkage agreement for East Side is its arrangement with the Santa Clara County GAIN program, which worked extensively with East Side to change the local GAIN contracting procedures and program flow to facilitate enrollment of New Chance participants. GAIN held special orientation sessions for potential New Chance applicants and referred new GAIN registrants to the program. New Chance participants who are co-enrolled in GAIN have child care, transportation, and GED books and tests paid for by GAIN, which also provides additional money for training materials or for tools and uniforms required for a job.

East Side faced the same challenges as most programs for hard-to-serve populations during the study period: participant punctuality, attendance, and retention. The site addressed these issues by using various "carrots" and "sticks." Participants received breakfast and lunch every day. A peer counseling program for which the participants elect the counselors was started. There were many field trips and regular awards luncheons honoring, for example, those who received or made progress toward receiving a GED, had shown a significant change in attitude, had near-perfect attendance, or were the best students "all around." The program coordinators also instituted a "Lucky Bucks" incentive program, whereby participants earned credits for being punctual, demonstrating leadership, volunteering to help others, and recruiting new applicants. These credits could be used to buy baby products, cosmetics, and toiletries from East Side. The attendance policy (participants must attend 65 percent of all scheduled classes) was strictly enforced, and there was a one-month probation period for those who did not meet the requirement.

Since the time period under study in this report, reduced funding has resulted in two rather significant changes. First, East Side has reduced the number of participants served each year by about 25 percent. Second, as per an agreement with the Santa Clara County GAIN program, participants return to their GAIN case manager after they complete the first phase of the program, which includes the education, life skills, parenting, family planning, and employability services. The GAIN case manager refers them to skills training centers or community colleges.

APPENDIX E

CHARACTERISTICS OF NEW CHANCE ENROLLEES, BY SITE

TABLE E.1

CHARACTERISTICS OF NEW CHANCE ENROLLEES, BY SITE

Characteristic and Subgroup at Random Assignment	All Sites	Allentown	Bronx	Chicago Heights	Chula Vista	Denver	Detroit	Harlem	Inglewood	Jacksonville	Lexington	Minneapolis	Philadelphia	Pittsburgh	Portland	Salem	San Jose		p (a)
Demographic characteristics																			
Age (years) (%)																		***	0.000
16	2.1	2.6	4.0	4.5	0.0	1.8	0.0	5.6	0.0	4.9	0.0	0.0	0.0	0.6	2.1	6.7	2.2		
17	17.5	17.4	10.3	13.4	11.0	12.7	27.2	11.3	20.6	30.6	20.7	11.6	31.1	12.3	14.7	12.7	15.7		
18	22.1	12.2	19.8	20.9	32.3	28.2	26.0	17.7	22.1	19.4	18.5	18.2	21.5	25.1	21.7	24.6	22.4		
19	25.6	27.0	27.0	26.9	21.3	29.1	21.9	16.9	21.4	22.9	19.3	31.4	31.1	25.7	40.6	23.1	26.1		
20	19.5	24.3	19.8	19.4	18.9	19.1	17.2	25.0	22.9	13.2	23.7	18.2	11.1	21.6	18.9	17.9	22.4		
21	11.1	14.8	18.3	10.4	11.0	7.3	7.7	18.5	13.0	9.0	14.8	14.0	5.2	14.6	2.1	11.2	7.5		
22	2.1	1.7	0.8	4.5	5.5	1.8	0.0	4.8	0.0	0.0	3.0	6.6	0.0	0.0	0.0	3.7	3.7		
Average age (years)	18.8	19.0	19.1	18.9	19.0	18.8	18.5	19.2	18.9	18.4	19.0	19.2	18.4	19.0	18.7	18.8	18.9	***	0.000
Ethnicity (%)																		***	0.000
Black, non-Hispanic	52.4	18.3	45.2	74.6	8.7	9.1	96.4	71.0	82.4	83.3	54.1	62.5	80.7	91.8	28.7	1.5	6.0		
Hispanic	22.3	42.6	54.8	6.0	61.4	58.2	0.0	28.2	13.7	0.0	0.7	3.3	8.9	1.2	8.4	23.1	64.2		
White	22.7	39.1	0.0	16.4	26.8	29.1	3.6	0.8	3.8	16.0	45.2	20.0	10.4	7.0	58.7	69.4	21.6		
Other	2.5	0.0	0.0	3.0	3.1	3.6	0.0	0.0	0.0	0.7	0.0	14.2	0.0	0.0	4.2	6.0	8.2		
Marital status (%)																		***	0.000
Never married	90.1	92.1	89.7	92.5	77.2	90.9	97.0	89.5	95.3	86.8	81.5	92.6	99.3	97.6	88.1	85.8	83.6		
Other	9.9	7.9	10.3	7.5	22.8	9.1	3.0	10.5	4.7	13.2	18.5	7.4	0.7	2.4	11.9	14.2	16.4		
Number of children (%)																		***	0.000
1	64.9	59.1	71.4	59.7	70.1	65.5	64.5	74.2	71.0	59.0	43.7	60.3	60.0	66.1	76.9	61.9	72.4		
2	26.8	29.6	23.0	26.9	26.0	31.8	29.6	21.0	26.0	27.1	34.1	28.1	34.1	24.6	18.2	26.9	23.1		
3 or more	8.3	11.3	5.6	13.4	3.9	2.7	5.9	4.8	3.1	13.9	22.2	11.6	5.9	9.3	4.9	11.2	4.5		
Average number of children	1.4	1.6	1.3	1.6	1.4	1.4	1.4	1.3	1.3	1.6	1.8	1.6	1.5	1.5	1.3	1.5	1.3	***	0.000
Age of youngest child (years) (%)																		***	0.000
Less than 1	53.8	62.6	34.1	55.1	48.0	50.9	65.1	44.4	41.2	61.1	62.2	47.1	54.8	51.5	60.8	66.4	50.0		
1	26.6	24.3	36.5	24.6	27.6	30.0	23.1	25.8	37.4	27.1	24.4	24.8	27.4	23.4	25.2	21.6	24.6		
2	12.1	7.8	19.0	13.0	15.7	10.0	8.9	18.5	16.8	9.0	4.4	14.9	12.6	11.7	10.5	7.5	14.9		
3 or older	7.5	5.2	10.3	7.2	8.7	9.1	3.0	11.3	4.6	2.8	8.9	13.2	5.2	13.5	3.5	4.5	10.4		
Average age of youngest child (years)	1.2	1.0	1.6	1.2	1.4	1.3	0.9	1.5	1.4	1.0	1.1	1.4	1.1	1.3	1.0	0.9	1.4	***	0.000

(continued)

BEST COPY AVAILABLE

TABLE E.1 (continued)

Characteristic and Subgroup at Random Assignment	All Sites	Allen-town	Bronx	Chicago Heights	Chula Vista	Denver	Detroit	Harlem	Ingle-wood	Jackson-ville	Lexing-ton	Minne-apolis	Phila-delphia	Pitts-burgh	Port-land	Salem	San Jose		p (a)
Age at first child's birth (years) (%)																		***	0.000
14 or under	5.3	4.3	3.2	8.7	2.4	4.5	3.6	4.0	2.3	11.8	9.6	5.0	11.1	6.4	2.1	2.2	3.7		
15	12.1	7.8	15.9	11.6	7.1	14.5	10.7	7.3	12.2	16.7	15.6	15.7	18.5	11.7	8.4	10.4	9.0		
16	22.9	21.7	19.8	21.7	26.0	22.7	27.2	17.7	25.2	29.9	20.7	20.7	24.4	18.7	22.4	21.6	23.9		
17	26.1	28.7	27.8	27.5	24.4	23.6	30.2	25.0	27.5	20.1	24.4	22.3	25.9	31.0	26.6	23.9	27.6		
18	21.6	20.9	18.3	21.7	32.3	22.7	19.5	24.2	16.8	11.1	19.3	26.4	13.3	22.2	25.2	30.6	22.4		
19	12.1	16.5	15.1	8.7	7.9	11.8	8.9	21.8	16.0	10.4	10.4	9.9	6.7	9.9	15.4	11.2	13.4		
Average age at first child's birth (years)	16.8	17.0	16.9	16.8	17.0	16.8	16.8	17.2	16.9	16.3	16.6	16.8	16.3	16.8	17.1	17.0	16.9	***	0.000
Living arrangement																			
Living with (%)																			
Mother	34.4	25.2	32.0	40.3	33.1	36.1	40.5	42.6	29.0	43.8	35.6	18.3	51.9	30.4	32.1	21.8	38.1	***	0.000
Father	7.6	9.6	2.5	13.4	13.4	13.9	8.3	9.0	3.8	0.7	6.7	5.0	3.1	4.7	11.2	9.0	12.7	***	0.000
Spouse or partner	11.6	14.8	5.7	14.9	15.0	9.3	6.0	9.0	8.4	10.4	14.1	10.8	11.5	5.3	19.4	27.8	7.5	***	0.000
No other adult	31.8	35.7	32.0	16.4	29.9	41.1	34.5	24.6	33.6	27.1	40.0	58.3	12.3	51.5	24.6	27.8	10.4	***	0.000
Lived in a female-headed household at age 14 (%)	48.9	28.7	51.6	46.4	31.5	31.8	48.5	58.1	54.2	59.0	50.4	54.5	65.2	70.8	49.7	31.3	37.3	***	0.000
Lived with both parents at age 14 (%)	22.2	36.5	18.3	20.9	29.9	30.9	24.0	20.3	16.0	9.7	25.9	19.0	15.0	11.3	22.2	31.8	29.5	***	0.000
Education characteristics																			
Highest grade completed (%)																		***	0.000
7th or below	3.0	6.1	1.6	0.0	2.4	3.6	1.2	0.8	0.0	11.2	7.4	0.8	1.5	0.0	2.1	4.4	3.7		
8th	10.3	15.7	6.3	4.5	11.0	11.8	3.0	8.9	3.8	15.3	13.3	7.4	17.0	4.7	12.6	17.9	11.2		
9th	22.8	27.0	34.1	13.4	26.0	21.8	17.2	17.7	11.5	29.2	17.8	21.5	40.7	14.1	23.8	24.6	23.9		
10th	30.3	22.6	33.3	23.9	36.6	29.1	33.7	34.7	30.5	26.4	35.6	28.9	24.4	27.1	30.1	31.3	31.3		
11th	27.8	27.0	21.4	44.8	15.7	24.5	44.4	32.3	47.3	17.4	23.7	40.5	15.6	31.8	17.5	20.9	25.4		
12th	5.8	1.7	3.2	13.4	6.3	9.1	0.6	5.6	6.9	0.7	2.2	0.8	0.7	22.4	14.0	0.7	4.5		
Average highest grade completed	9.9	9.5	9.8	10.5	9.7	9.9	10.2	10.1	10.4	9.2	9.6	10.0	9.4	10.5	9.9	9.5	9.8	***	0.000
Received high school diploma or GED (%)	6.3	1.7	0.0	13.4	7.1	12.7	0.6	8.1	4.6	2.1	4.4	0.0	0.0	20.6	22.4	3.0	0.7	***	0.000
Left school before first pregnancy (%)	37.2	45.2	35.7	14.9	51.2	40.9	33.7	39.5	26.0	31.3	29.6	43.0	24.4	21.2	44.1	62.7	49.3	***	0.000

(continued)

TABLE E.1 (continued)

Characteristic and Subgroup at Random Assignment	All Sites	Allen-town	Bronx	Chicago Heights	Chula Vista	Denver	Detroit	Harlem	Ingle-wood	Jackson-ville	Lexing-ton	Minne-apolis	Phila-delphia	Pitts-burgh	Port-land	Salem	San Jose		p (a)
Average number of years since last attended school	2.4	2.7	3.1	2.2	2.7	2.4	1.7	2.6	2.1	2.1	2.5	2.4	2.3	2.0	2.2	2.6	2.6	***	0.000
Reading level (grade equivalent) (%)																		***	0.000
4th grade or below	8.5	10.2	13.3	14.7	10.0	2.9	9.3	0.0	6.0	18.6	10.2	0.0	16.5	1.2	11.9	10.6	2.4		
5th grade	5.8	8.3	7.1	2.9	7.5	7.8	8.7	0.8	9.5	7.0	7.1	0.9	8.7	0.6	5.2	5.7	4.0		
6th grade	9.4	12.0	15.9	8.8	8.3	10.8	18.6	0.8	8.6	10.9	10.2	4.7	12.6	5.5	7.5	6.5	6.4		
7th grade	10.8	5.6	13.3	17.6	7.5	9.8	8.7	4.2	12.9	14.7	7.9	18.7	14.2	19.0	6.0	4.9	8.8		
8th grade	14.0	15.7	15.0	8.8	11.7	15.7	13.0	24.6	11.2	11.6	14.2	15.0	8.7	22.7	7.5	8.1	17.6		
9th grade	21.4	17.6	18.6	19.1	20.8	23.5	12.4	44.1	20.7	17.8	19.7	20.6	11.8	31.9	24.6	19.5	19.2		
10th grade or above	30.1	30.6	16.8	27.9	34.2	29.4	29.2	25.4	31.0	19.4	30.7	40.2	27.6	19.0	37.3	44.7	41.6		
Average reading level (grade equivalent)	8.4	8.3	7.4	8.1	8.5	8.6	7.8	9.0	8.5	7.5	8.2	9.4	7.6	8.3	8.7	9.0	9.2	***	0.000
Desired educational attainment for self (%)																			
High school diploma or GED	32.6	35.7	34.9	23.9	44.9	23.6	38.7	32.5	26.7	38.9	30.4	27.3	47.4	20.1	26.8	32.1	34.3	***	0.000
1–3 years of college (A.A. degree)	31.2	27.8	19.8	32.8	28.3	45.5	17.3	19.5	39.7	34.7	25.2	42.1	20.0	37.3	38.0	41.8	32.8		
4 years of college (B.A. degree)	22.2	17.4	34.9	23.9	15.0	20.9	24.4	35.8	14.5	16.0	25.9	19.0	23.0	32.0	21.8	13.4	16.4		
Graduate degree	10.8	15.7	9.5	17.9	5.5	10.0	19.6	8.9	15.3	9.7	11.1	7.4	5.2	7.1	9.2	10.4	11.9		
Other	3.2	3.5	0.8	1.5	6.3	0.0	0.0	3.3	3.8	0.7	7.4	4.1	4.4	3.6	4.2	2.2	4.5		
Desired educational attainment for child (b) (%)																		***	0.000
Elementary school	0.2	0.0	0.0	0.0	0.0	0.0	0.6	0.0	0.0	0.7	0.0	0.0	0.0	0.6	0.7	0.0	0.0		
High school	20.8	26.1	16.0	19.4	16.5	26.4	17.2	9.7	16.3	31.2	17.8	19.5	24.1	16.4	23.4	27.6	24.6		
College/post-secondary	57.8	47.8	63.2	70.1	70.9	50.9	46.7	69.0	61.2	50.4	56.3	51.7	65.4	69.6	48.9	54.5	54.5		
Graduate school	21.2	26.1	20.8	10.4	12.6	22.7	35.5	21.2	22.5	17.7	25.9	28.8	10.5	13.5	27.0	17.9	20.9		
Mother has high school diploma or GED (%)	52.5	41.7	36.5	61.2	41.7	50.0	66.9	55.6	63.4	49.3	48.1	54.5	51.1	59.6	60.6	47.0	48.5	***	0.000
Mother attended college (%)	25.1	21.7	18.3	25.4	22.0	20.9	34.9	29.0	48.1	16.7	17.0	31.4	11.9	19.9	30.1	31.3	21.6	***	0.000
Father has high school diploma or GED (%)	43.1	30.4	36.5	59.7	38.6	43.6	49.7	38.7	45.8	38.9	35.6	43.8	42.2	48.5	51.7	43.3	45.5	***	0.000
Father attended college (%)	16.2	10.4	10.3	17.9	18.9	20.9	17.2	14.5	19.8	11.1	13.3	20.7	8.9	12.4	26.6	17.9	19.4	***	0.000

(continued)

BEST COPY AVAILABLE

TABLE E.1 (continued)

Characteristic and Subgroup at Random Assignment	All Sites	Allen-town	Bronx	Chicago Heights	Chula Vista	Denver	Detroit	Harlem	Ingle-wood	Jackson-ville	Lexing-ton	Minne-apolis	Phila-delphia	Pitts-burgh	Port-land	Salem	San Jose		p (a)
Both parents have high school diplomas or GEDs (%)	29.4	16.5	19.0	46.3	22.8	28.2	40.2	30.6	35.9	27.8	23.7	26.4	24.4	33.3	36.6	27.6	32.1	***	0.000
Both parents attended college (%)	7.3	5.2	2.4	10.4	6.3	10.9	8.9	8.1	12.2	2.8	4.4	9.1	0.7	5.3	12.6	11.9	7.5	***	0.000
Employment and welfare receipt																			
Number of jobs ever held (%)																		***	0.000
0	21.5	11.3	22.2	24.6	35.4	16.4	21.9	12.9	26.0	22.2	8.9	18.2	15.6	42.7	16.1	12.7	29.9		
1-2	32.7	26.1	23.8	30.4	36.2	32.8	45.6	34.7	42.0	38.9	31.1	28.9	32.6	27.5	27.3	29.1	32.1		
3 or more	45.8	62.6	54.0	44.9	28.3	51.0	32.6	52.4	32.0	38.9	60.0	52.9	51.9	29.8	56.7	58.2	38.1		
Average number of jobs held	4.1	4.2	7.8	2.3	3.2	3.0	2.6	3.0	4.1	2.6	3.5	7.1	7.6	3.4	5.2	3.8	2.2	***	0.000
Employed at random assignment (%)	3.1	0.0	0.8	4.5	0.0	2.7	2.4	3.3	2.3	3.5	8.1	5.0	3.7	5.9	2.3	3.7	0.8	***	0.008
Number of months employed in prior 12 months (%)																		***	0.000
0	63.3	63.5	77.0	56.5	76.4	61.8	70.4	63.7	73.8	57.6	45.9	58.7	60.0	70.6	52.1	50.0	70.1		
3 or less	18.4	21.8	9.5	20.3	10.2	16.3	15.4	14.5	16.9	19.5	28.1	16.6	23.7	14.2	26.0	29.8	11.9		
4-6	10.0	7.8	7.1	10.1	7.1	12.7	8.9	13.7	6.2	12.5	15.6	12.4	11.1	8.8	9.2	7.5	9.7		
7-12	8.0	7.0	5.6	13.0	6.3	9.1	4.7	8.1	3.1	10.4	10.4	9.9	4.4	5.9	12.0	12.7	8.2		
Prior-year earnings (%)																		***	0.000
\$0-\$500	79.9	80.0	86.4	69.6	82.7	78.2	89.3	77.4	85.5	76.4	65.9	80.5	85.1	85.9	73.8	71.6	82.0		
\$501 or more	20.1	20.0	13.6	30.4	17.3	21.8	10.7	22.6	14.5	23.6	34.1	19.5	14.9	14.1	26.2	28.4	18.0		
Length of longest job (%)																		***	0.000
Never employed	21.2	10.4	22.4	24.6	35.4	16.4	20.7	12.9	25.2	21.5	8.9	18.5	15.6	43.5	16.2	12.7	29.1		
Less than 1 month	3.7	4.3	1.6	0.0	3.9	3.6	5.9	4.8	5.3	6.9	4.4	0.8	1.5	0.6	2.1	5.2	5.2		
1-3 months	22.7	23.5	20.0	15.9	17.3	21.8	26.6	25.0	25.2	17.4	25.9	25.2	33.3	22.0	23.9	22.4	13.4		
4-6 months	22.3	20.0	20.0	29.0	15.0	18.2	19.5	25.8	26.7	25.0	23.0	21.0	28.1	18.5	26.1	27.6	16.4		
7-12 months	17.9	23.5	19.2	15.9	18.1	23.6	16.6	15.3	15.3	21.5	23.0	16.8	12.6	8.3	20.4	17.2	22.4		
Over 1 year	12.2	18.3	16.8	14.5	10.2	16.4	10.7	16.1	2.3	7.6	14.8	17.6	8.9	7.1	11.3	14.9	13.4		
Mother employed (%)																		***	0.000
Yes	49.8	49.6	28.6	46.3	41.7	65.5	43.8	42.7	58.0	52.8	65.9	51.2	36.3	43.3	58.0	56.7	57.5		
No	42.2	45.2	61.9	43.3	46.5	31.8	44.4	46.8	36.6	38.9	31.1	38.0	57.8	48.5	34.3	37.3	31.3		
Don't know	4.0	1.7	0.8	3.0	6.3	1.8	6.5	5.6	2.3	3.5	1.5	6.6	3.0	3.5	5.6	4.5	6.7		
Deceased	4.0	3.5	8.7	7.5	5.5	0.9	5.3	4.8	3.1	4.9	1.5	4.1	3.0	4.7	2.1	1.5	4.5		

(continued)

TABLE E.1 (continued)

Characteristic and Subgroup at Random Assignment	All Sites	Allen–town	Bronx	Chicago Heights	Chula Vista	Denver	Detroit	Harlem	Ingle–wood	Jackson–ville	Lexing–ton	Minne–apolis	Phila–delphia	Pitts–burgh	Port–land	Salem	San Jose		p (a)
Father employed (%)																		***	0.000
Yes	45.4	43.5	37.3	49.3	52.0	52.7	35.5	32.3	53.4	46.5	54.8	45.5	39.3	34.9	51.0	50.0	55.2		
No	19.8	20.9	26.2	20.9	18.9	16.4	24.3	21.0	13.0	12.5	18.5	16.5	17.8	29.6	23.1	19.4	14.2		
Don't know	25.0	26.1	23.8	16.4	22.0	27.3	27.2	35.5	26.0	29.9	19.3	24.0	33.3	23.1	18.2	22.4	22.4		
Deceased	9.8	9.6	12.7	13.4	7.1	3.6	13.0	11.3	7.6	11.1	7.4	14.0	9.6	12.4	7.7	8.2	8.2		
Receives AFDC (%)																		***	0.000
Own grant	87.4	93.0	86.5	85.1	99.2	84.5	79.9	68.5	97.7	77.1	83.0	98.3	76.3	91.8	94.4	85.1	98.5		
Other person's grant	7.4	2.6	13.5	4.5	0.8	0.9	15.4	18.5	2.3	12.5	6.7	1.7	18.5	8.2	3.5	1.5	1.5		
Not receiving AFDC	5.2	4.3	0.0	10.4	0.0	14.5	4.7	12.9	0.0	10.4	10.4	0.0	5.2	0.0	2.1	13.4	0.0		
Receives (%)																			
Medicaid	87.1	97.4	99.2	83.6	96.1	85.3	92.9	86.2	87.6	89.4	94.1	51.3	81.2	89.3	74.1	85.7	94.8	***	0.000
Food stamps	83.7	75.7	90.5	76.1	80.3	82.7	83.9	71.3	70.5	75.5	87.4	97.5	75.9	96.4	89.5	88.0	88.8	***	0.000
Public housing	23.2	29.6	25.4	14.9	4.7	24.5	8.9	39.0	1.5	27.1	52.6	9.2	24.8	55.6	11.9	28.4	4.5	***	0.000
Income from a job	3.5	0.0	1.6	4.5	0.8	4.5	4.7	4.9	1.5	3.5	7.4	4.2	4.5	4.1	4.2	4.5	0.0		0.164
Family received AFDC when sample member was growing up (%)																		***	0.000
Always	16.6	14.8	48.4	10.6	6.3	4.5	18.5	27.7	11.5	7.0	9.6	24.8	24.1	27.5	9.2	8.3	8.3		
2 years or less (c)	18.7	21.7	5.6	19.7	19.7	26.4	20.8	11.8	20.6	16.1	19.3	15.7	18.0	7.6	24.6	27.8	26.5		
More than 2 years, but not always (c)	28.5	26.1	27.0	27.3	32.3	12.7	27.4	26.1	30.5	36.4	22.2	32.2	38.3	39.2	27.5	20.3	23.5		
Never	36.2	37.4	19.0	42.4	41.7	56.4	33.3	34.5	37.4	40.6	48.9	27.3	19.5	25.7	38.7	43.6	41.7		
Fertility–related characteristics																			
Number of pregnancies (%)																			0.405
1	43.4	46.1	34.1	43.3	46.5	44.5	42.6	44.4	38.9	46.5	36.6	42.0	43.3	43.9	46.8	46.3	47.8		
2	32.2	33.9	36.5	29.9	34.6	38.2	32.5	29.8	34.4	29.9	32.8	26.1	34.3	31.0	35.5	25.4	30.6		
3	16.1	10.4	14.3	19.4	14.2	13.6	15.4	15.3	17.6	18.8	22.4	17.6	17.2	17.0	11.3	17.9	14.9		
4	6.0	7.8	11.1	6.0	1.6	2.7	5.9	7.3	4.6	4.2	6.7	10.1	3.7	7.6	5.7	6.0	4.5		
5 or more	2.4	1.7	4.0	1.5	3.1	0.9	3.6	3.2	4.6	0.7	1.5	4.2	1.5	0.6	0.7	4.5	2.2		
Average number of pregnancies	1.9	1.9	2.2	1.9	1.8	1.8	2.0	2.0	2.1	1.8	2.0	2.1	1.9	1.9	1.8	2.0	1.9	*	0.068
Ever had an abortion (%)	23.1	14.8	41.3	11.9	24.4	20.9	27.8	33.9	38.9	7.6	8.9	22.3	14.8	25.1	28.0	14.2	28.4	***	0.000
When next child is expected (%)																		***	0.000
Not expecting another child	64.2	60.0	67.5	77.6	51.6	64.5	72.9	65.6	59.2	83.9	79.3	69.9	60.7	59.1	55.9	48.1	55.6		
Within 2 years	1.7	0.0	2.4	1.5	1.6	1.8	1.8	0.8	1.5	0.7	0.0	0.9	0.0	2.3	1.5	6.8	2.3		
In 2–4 years	16.9	20.1	17.1	10.5	21.4	14.5	9.0	15.7	15.4	7.7	8.9	18.6	11.1	19.3	28.0	27.9	24.8		
In 5 years or more	17.3	20.0	13.0	10.5	25.4	19.1	16.2	18.0	23.8	7.7	11.9	10.6	28.2	19.3	14.7	17.3	17.3		

(continued)

TABLE E.1 (continued)

Characteristic and Subgroup at Random Assignment	All Sites	Allentown	Bronx	Chicago Heights	Chula Vista	Denver	Detroit	Harlem	Inglewood	Jacksonville	Lexington	Minneapolis	Philadelphia	Pittsburgh	Portland	Salem	San Jose		p (a)
Average number of years until next child is expected (d)	4.4	4.5	3.5	4.8	4.9	4.5	5.0	4.4	5.3	5.0	4.6	3.4	5.7	4.5	3.9	3.7	4.1	***	0.000
Current birth control use (%)																		***	0.000
Yes, using birth control	62.4	60.9	54.8	50.7	59.1	67.3	69.8	57.3	50.4	66.0	71.9	68.6	63.0	64.3	65.5	62.7	57.5		
No, not using birth control	12.1	11.3	21.4	11.9	9.4	4.5	11.2	15.3	20.6	12.5	7.4	9.9	13.3	11.7	7.0	14.2	11.9		
No partner/not having sex	25.5	27.8	23.8	37.3	31.5	28.2	19.0	27.5	29.0	21.6	20.8	21.5	23.7	24.0	27.5	23.1	30.6		
Used birth control at last intercourse (%)	71.0	63.5	61.9	56.7	73.2	72.7	73.4	66.9	63.4	81.1	80.7	75.0	65.9	66.5	85.3	67.9	71.6	***	0.000
Relations with child's father																			
Speaks with child's father (b) (%)	67.5	66.1	69.0	68.7	57.5	45.5	80.5	76.4	73.8	60.4	68.9	63.6	77.8	72.5	66.4	61.2	64.2	***	0.000
Has child support order (b) (%)	27.9	67.8	16.7	19.4	15.1	18.2	32.0	13.8	9.2	22.9	34.3	23.1	32.6	56.1	25.2	22.4	25.6	***	0.000
Prior and current service receipt																			
Ever in occupational skills training (%)	22.3	33.9	27.8	10.4	25.2	10.9	21.9	39.5	28.2	30.1	10.4	16.5	26.7	22.8	21.1	12.7	13.4	***	0.000
Services received in the 60 days before random assignment (%)																			
Health care for child	84.6	85.2	86.5	70.1	87.4	84.1	85.8	87.1	80.9	76.6	85.9	77.7	87.3	93.6	82.4	88.8	83.6	***	0.001
Family planning	22.9	32.2	27.0	28.4	17.3	19.6	13.0	42.7	12.2	43.1	14.1	5.8	36.6	24.6	12.7	23.9	18.7	***	0.000
Mental health	2.7	1.7	3.2	1.5	1.6	2.8	2.4	6.5	2.3	2.2	3.7	1.7	3.0	2.9	3.5	1.5	2.2		0.770
Health care for self	58.7	68.7	60.3	56.7	50.4	47.7	66.9	58.1	49.6	46.7	48.1	53.7	67.9	82.5	59.2	62.7	48.5	***	0.000
Parenting	11.0	13.9	12.7	19.4	7.9	6.5	4.7	23.4	6.9	14.6	10.4	14.0	14.9	2.3	8.5	16.4	8.2	***	0.000
Life skills	2.9	6.1	2.4	6.0	3.1	1.9	1.8	8.1	0.8	6.6	1.5	0.0	1.5	0.0	4.2	3.7	1.5	***	0.000
Counseling	4.1	4.3	2.4	4.5	2.4	3.7	1.8	10.5	2.3	8.0	1.5	2.5	3.7	0.0	2.8	7.5	9.0	***	0.000
Other services	10.5	55.7	1.6	0.0	3.9	15.9	1.8	12.1	2.3	3.6	0.0	3.3	1.5	32.7	4.2	9.7	17.2	***	0.000
No services	8.4	3.5	10.3	10.4	8.7	7.5	6.5	7.3	11.5	11.7	7.4	19.8	6.7	2.9	11.3	4.5	8.2	***	0.000
Has regular child care (e) (%)	43.7	40.9	60.3	50.7	48.0	38.2	60.9	63.7	32.8	37.5	20.7	51.2	64.4	14.2	54.5	45.9	22.7	***	0.000
Psychosocial characteristics																			
CES-D (depression) Scale (f) (%)																		***	0.000
0-15 (not at risk)	47.0	43.5	45.2	43.3	55.9	55.5	41.4	49.2	52.7	35.9	51.1	43.8	33.3	52.6	61.5	36.6	49.3		
16-23 (at some risk)	25.9	33.0	24.6	23.9	22.8	20.9	25.4	31.5	19.1	21.8	24.4	30.6	31.1	29.8	18.2	26.1	29.9		
24-60 (at high risk)	27.2	23.5	30.2	32.8	21.3	23.6	33.1	19.4	28.2	42.3	24.4	25.6	35.6	17.5	20.3	37.3	20.9		
Average CES-D score (f)	18.1	18.1	19.0	18.7	16.3	16.4	19.6	16.3	17.3	21.7	18.3	18.1	19.9	16.0	15.8	20.9	16.5	***	0.000

(continued)

TABLE E.1 (continued)

Characteristic and Subgroup at Random Assignment	All Sites	Allen-town	Bronx	Chicago Heights	Chula Vista	Denver	Detroit	Harlem	Ingle-wood	Jackson-ville	Lexing-ton	Minne-apolis	Phila-delphia	Pitts-burgh	Port-land	Salem	San Jose	p (a)
Average number of sources of emotional support	2.7	2.0	2.6	1.5	2.4	2.4	2.8	4.5	2.3	3.1	2.2	3.0	2.3	3.3	3.5	3.3	1.9 ***	0.000
Average level of satisfaction with emotional support (g)	4.2	4.2	4.3	4.4	4.3	4.3	4.4	4.2	4.0	3.9	4.0	4.0	4.2	4.4	3.9	3.9	4.2 ***	0.000
Average self-esteem score (h)	38.3	36.9	37.6	39.8	37.9	38.9	38.4	39.8	39.2	38.1	37.4	40.1	37.5	39.7	39.4	35.3	37.7 ***	0.000
Average Locus of Control score (i)	22.0	21.8	21.7	22.5	22.4	22.7	21.4	22.5	22.1	21.4	22.0	22.9	21.8	21.6	22.5	21.9	21.9 ***	0.002
Other																		
Has home telephone (%)	83.9	82.6	79.4	85.1	87.4	84.5	94.7	71.8	98.5	80.6	60.7	83.5	89.6	98.8	81.1	70.9	87.3 ***	0.000
Has driver's license (%)	27.9	15.7	3.2	32.8	54.3	48.2	32.5	1.6	19.1	58.3	23.7	14.9	1.5	0.6	27.3	73.9	43.3 ***	0.000
Sample size	2,088	115	126	69	127	110	169	124	131	144	,135	121	135	171	143	134	134	

SOURCE: MDRC calculations from New Chance Enrollment Form data.

NOTES: Calculations for this table used data for all 2,088 sample members for whom there were 18 months of follow-up survey data, including those with values of zero for outcomes and New Chance enrollees (i.e., experimentals) who did not participate in the program. The reported sample sizes may fall short of this number because of missing or unusable items from some sample members' questionnaires.

(a) A t-test or F-test was applied to each difference in characteristics between sites. The column labeled "p" is the statistical significance level of these differences. That is, p is the probability that these differences exist only because of random error. Statistical significance levels are indicated as *** = 1 percent; ** = 5 percent; * = 10 percent.

(b) When a sample member had more than 1 child, her response refers to her first child.

(c) The family's AFDC receipt may not have been continuous.

(d) Includes only those sample members who expected to have more children.

(e) Regular child care was defined as an ongoing arrangement used while the mother was in school, in training, or working.

(f) The Center for Epidemiological Studies Depression (CES-D) Scale is a widely used measure of depression; scores can range from zero to 60.

(g) Enrollees were also asked about their degree [illegible] satisfaction with the emotional support ("people who listen to you, reassure you, and show you they care") they received. Levels range from 1 (very dissatisfied) to 5 (very satisfied).

(h) The measure of self-esteem used was the Rosenberg Self-Esteem Scale, a 10-item scale that assesses a person's global sense of self-worth. Scores can range from 10 to 50; 30 is considered the neutral midpoint.

(i) The Locus of Control Scale is a six-item adaptation of the longer scale originally developed by Julien Rotter (1966). Scores can range from 6 to 30; 18 is considered the neutral midpoint.

REFERENCES

Adams, G. 1990. *Sources of Support for Adolescent Mothers*. Washington, D.C.: Congressional Budget Office.

Ahn, N. 1994. "Teenage Childbearing and High School Completion: Accounting for Individual Heterogeneity." *Family Planning Perspectives*. 26: 17-21.

Auspos, P., Cave, G., Doolittle, F., and Hoerz. G. 1989. *Implementing JOBSTART: A Demonstration for School Dropouts in the JTPA System*. New York: Manpower Demonstration Research Corporation.

Baker, P., and Mott, F. 1989. *NLSY Child Handbook: A Guide and Resource Document for the National Longitudinal Survey of Youth 1986 Child Data*. Columbus, Ohio: Center for Human Resource Research.

Bane, M. J., and Ellwood, D. T. 1983. *The Dynamics of Dependence: The Routes to Self-Sufficiency*. Cambridge, Mass.: Urban Systems Research and Engineering.

Becker, G. S. 1974. *Human Capital*. New York: National Bureau of Economic Research.

Belle, D. 1982. *Lives in Stress: Women and Depression*. Beverly Hills, Calif.: Sage.

Belsky, J. 1990. "Parental and Nonparental Care and Children's Socioemotional Development: A Decade in Review." *Journal of Marriage and the Family* 52: 885-903.

Belsky, J., and Eggebeen, D. 1991. "Early and Extensive Maternal Employment and Young Children's Socioemotional Development." *Journal of Marriage and the Family* 53: 1083-110.

Berg, I. 1969. *Education and Jobs: The Great Training Robbery*. New York: Praeger.

Berlin, G., and Sum, A. 1988. *Toward a More Perfect Union: Basic Skills, Poor Families, and Our Economic Future*. New York: Ford Foundation.

Blackburn, M., Bloom, D., and Freeman, R. 1989. "Why Has the Economic Position of Less-Skilled Male Workers Deteriorated in the United States?" Mimeo.

Blau, D. M, and Robins, P. K. 1989. "Fertility, Employment, and Child Care Costs." *Demography* 26: 287-300.

Bloom, D., Fellerath, V., Long, D., and Wood, R. G. 1993. *LEAP: Interim Findings on a Welfare Initiative to Improve School Attendance Among Teenage Parents*. New York: Manpower Demonstration Research Corporation.

Bloom, H. S. 1984. "Accounting for No-Shows in Experimental Evaluation Designs." *Evaluation Review* 8 (2): 225-46.

Bloom, H. S., Orr, L. A., Cave, G., Bell, S. H., and Doolittle, F. 1993. *The National JTPA Study: Title II-A Impacts on Earnings and Employment at 18 Months*. Bethesda, Md.: Abt Associates Inc.

Booth, C. L., Mitchell, S. K., Barnard, K. E., and Spieker, S. J. 1989. "Development of Maternal Social Skills in Multiproblem Families: Effects on the Mother-Child Relationship." *Developmental Psychology* 25: 403-12.

Bredekamp, S., ed. 1984. *Accreditation Criteria and Procedures of the National Academy of Early Childhood Programs*. Washington, D.C.: National Association for the Education of Young Children.

Brooks-Gunn, J., and Berlin, L. 1993. "A Consideration of Self-Sufficiency and Parenting in the Context of the Teenage Parent Demonstration Program." Paper presented at the 2d National Head Start Research Conference, November, Washington, D.C.

Brooks-Gunn, J., and Furstenberg, F. 1986. "The Children of Adolescent Mothers: Physical, Academic, and Psychological Outcomes." *Developmental Review* 6: 224-51.

Brown, H., Adams, R. G., and Kellam, S. G. 1981. "A Longitudinal Study of Teenage Motherhood and Symptoms of Distress." In *Research in Community Mental Health*. Vol. 3, ed. R. Simmons. Greenwich, Conn.: JAI Press.

Burchinal, M., Lee, M., and Ramey, C. 1989. "Type of Day-Care and Preschool Intellectual Development in Disadvantaged Children." *Child Development* 60: 128-37.

Caldwell, B., and Bradley, R. 1984. *Home Observation for Measurement of the Environment*. Little Rock: University of Arkansas.

Cameron, S. V., and Heckman, J. J. 1993. "The Nonequivalence of High School Equivalents." *Journal of Labor Economics* 11 (1): 1-47.

Catania, J. A. 1993. "Prevalence of AIDS-Related Risk Factors and Condom Use in the United States." *Science* 258: 1101-6.

Cave, G., 1987. "Sample Sizes for Social Experiments." In *Proceedings of the Section on Survey Research Methods of the American Statistical Association.*

Cave, G., and Bos, H. 1994. "The Value of a GED in a Choice-Based Experimental Sample." Unpublished paper.

Cave, G., Bos, H., Doolittle, F., and Toussaint, C. 1993. *JOBSTART: Final Report on a Program for School Dropouts*. New York: Manpower Demonstration Research Corporation.

Cave, G., and Doolittle, F. 1991. *Assessing JOBSTART: Interim Impacts of a Program for School Dropouts*. New York: Manpower Demonstration Research Corporation.

Center for Population Options. 1993. *The Cost Study Update*. Washington, D.C.: Center for Population Options.

Coates, D. L., and Lewis, M. 1984. "Early Mother-Infant Interactions and Infant Cognitive Status as Predictors of School Performance and Cognitive Behavior in Six-Year-Olds." *Child Development* 55: 1219-30.

Colletta, N. D. 1983. "At Risk for Depression: A Study of Young Mothers." *Journal of Genetic Psychology* 142: 301-10.

Cooley, M. L., and Unger, D. G. 1991. "The Role of Family Support in Determining Developmental Outcomes in Children of Teen Mothers." *Child Psychiatry and Human Development* 21: 217-34.

D'Amico, R. J., Haurin, R. J., and Mott, F. L. 1983. "The Effects of Mothers' Employment on Adolescent and Early Adult Outcomes of Young Men and Women." In *Children of Working Parents: Experiences and Outcomes*, ed. C. D. Hayes and S. B. Kamerman (130-219). Washington, D.C.: National Academy Press.

Danziger, S. 1991. "The Poor." In *Human Capital and America's Future,* ed. D. W. Hornbeck and L. M. Salamon (139-67). Baltimore and London: Johns Hopkins University Press.

DaVanzo, J., and Goldsheider, F. 1990. "Coming Home Again: Returns to the Parental Home of Young Adults." *Population Studies* 44: 241-55.

Dawson, D. A. 1991. "Family Structure and Children's Health and Well-Being." *Journal of Marriage and the Family* 53: 573-84.

Denham, S. A., Renwick, S. M., and Holt, R. W. 1991. "Working and Playing Together: Prediction of Preschool Socioemotional Competence from Mother-Child Interaction." *Child Development* 62: 242-49.

Desai, S., Michael, R. T., and Chase-Lansdale, P. L. 1990. *The Home Environment: A Mechanism Through Which Maternal Employment Affects Child Development*. Chicago: The Population Research Center, Economic Demography Group, Economics Research Center, NORC/University of Chicago. Unpublished.

Duncan, G. J., and Hoffman, S. D. 1990. "Teenage Welfare Receipt and Subsequent Dependence Among Black Adolescent Mothers." *Family Planning Perspectives* 16: 16-20.

Eck, A. 1993. "Job-Related Education and Training: Their Impact on Earnings." *Monthly Labor Review* 116 (10): 21-38.

Ekstrom, R. B., Goertz, M. E., Pollack, J. M., and Rock, D. A. 1986. "Who Drops Out of High School and Why? Findings from a National Study." *Teachers College Record* 87 (3): 356-73.

Ellwood, D. T. 1986. *Targeting "Would-Be" Long-Term Recipients of AFDC*. Princeton, N.J.: Mathematica Policy Research.

Emerick, S. J., Foster, L. R., and Campbell, D. T. 1986. "Risk Factors for Traumatic Infant Death in Oregon, 1973-1982." *Pediatrics* 77: 518-22.

Entwistle, D. R., and Alexander, K. L. 1990. "Beginning School Math Competence: Minority and Majority Comparisons." *Child Development* 61: 454-71.

Estrada, P., Arsenio, W. F., Hess, R. D., and Holloway, S. D. 1987. "Affective Quality of the Mother-Child Relationship: Longitudinal Consequences for Children's School-Relevant Cognitive Functioning.", *Developmental Psychology* 23: 210-15.

Fink, B. 1994. *New Chance: The Cost of Analysis of a Comprehensive Program for Disadvantaged Young Mothers and Their Children*. Forthcoming.

Fox, N., and Fein, G., eds. 1990. *Infant Day Care: The Current Debate*. Norwood, N.J.: Ablex.

Friedlander, D., and Burtless, G. Forthcoming. *Five Years After: The Long-Term Effects of Welfare-to-Work Programs*. New York: Russell Sage Foundation.

Furstenberg, Jr., F. F. 1991. "As the Pendulum Swings: Teenage Childbearing and Social Concern." *Family Relations* 40: 127-38.

Furstenberg, Jr., F. F., Brooks-Gunn, J., and Morgan, S. P. 1987. *Adolescent Mothers in Later Life*. Cambridge: Cambridge University Press.

Garcia-Coll, C. T. 1990. "Developmental Outcome of Minority Infants: A Process-Oriented Look into Our Beginnings." *Child Development* 61: 270-89.

Garcia-Coll, C. T., Hoffman, J., Oh, W. 1987. "The Social Ecology and Early Parenting of Caucasian Adolescent Mothers." *Child Development* 58: 955-63.

Garrett, P., Ferron, J., Ng'andu, N., Bryant, D., and Harbin, G. 1994. "A Structural Model for the Developmental Status of Young Children." *Journal of Marriage and the Family* 56: 147-63.

Geronimus, A., and Korenman, S. 1992. "The Socioeconomic Consequences of Teen Childbearing Reconsidered." *Quarterly Journal of Economics* 107: 1187-214.

Geronimus, A., and Korenman, S. 1993. "The Socioeconomic Costs of Teenage Childbearing: Evidence and Interpretation." *Demography* 30: 281-90.

Gladstein, J., Rusonis, E., and Heald, F. 1992. "A Comparison of Inner-City and Upper-Middle-Class Youths' Exposure to Violence." *Journal of Adolescent Health* 13: 275-80.

Gordon, A., and Burghardt, J. 1990. *The Minority Female Single Parent Demonstration: Short-Term Economic Impacts*. New York: The Rockefeller Foundation.

Grogger, J., and Bronars, S. 1993. "The Socioeconomic Consequences of Teenage Childbearing: Findings from a Natural Experiment." *Family Planning Perspectives* 25: 156-61.

Gueron, J. M., and Pauly, E. 1991. *From Welfare to Work*. New York: Russell Sage Foundation.

Haggstrom, G. W., Blaschke, T. J., Kanouse, D. E., Lisowski, W., and Morrison, P. A. 1981. *Teenage Parents: Their Ambitions and Attainments*. Santa Monica, Calif.: Rand Corporation.

Hannan, K. and Luster, T. 1991. "Influence of Parent, Child, and Contextual Factors on the Quality of the Home Environment." *Infant Mental Health Journal* 12: 17-30.

Harms, T., and Clifford, R. 1980. *Early Childhood Environment Rating Scale*. New York: Teachers College Press.

Harms, T., Cryer, D., and Clifford, R. 1990. *Infant/Toddler Environment Rating Scale*. New York: Teachers College Press.

Heckman, J. J., Hotz, V. J., and Walker, J. R. 1985. "The Influence of Early Fertility on Subsequent Births and the Importance of Controlling for Unobserved Heterogeneity." *Bulletin of the International Statistical Institute* 51: 1-16.

Higgins, C. 1988. *Youth Motivation: At-Risk Youth Talk to Program Planners*. Philadelphia: Public/Private Ventures.

Hofferth, S. L. 1987. "Social and Economic Consequences of Teenage Childbearing." In *Risking the Future: Adolescent Sexuality, Pregnancy, and Childbearing,* ed. S. L. Hofferth and C. D. Hayes, vol. 2 (123-44). Washington, D.C.: National Academy Press.

Hofferth, S. L., Brayfield, A., Delch, S., and Holcomb, P. 1991. *National Child Care Survey, 1990*. Washington, D.C.: Urban Institute.

Hofferth, S. L., Moore, K. A., and Caldwell, S. 1978. *The Consequences of Age at First Childbirth: Labor Force Participation and Earning*. Washington, D.C.: Urban Institute.

Hoffman, S. D., Foster, E. M., and Furstenberg, F. F. 1993. "Reevaluating the Costs of Teenage Childbearing." *Demography* 30: 1-12.

Horwitz, S. M., Klerman, L. V., Kuo, H. S., and Jekel, J. F. 1991. "Intergenerational Transmission of School-age Parenthood." *Family Planning Perspectives* 23: 168-72.

Hughes, D., Johnson, K., Rosenbaum, S., and Liu, J. 1989. *The Health of America's Children*. Washington, D.C.: Children's Defense Fund.

Hunter, A. G., and Ensminger, M. E. 1992. "Diversity and Fluidity in Children's Living Arrangements: Family Transitions in an Urban Afro-American Community." *Journal of Marriage and the Family* 54: 418-26.

Jones, E., and Forrest, J. D. 1992. "Underreporting of Abortion in Surveys of U.S. Women." *Demography* 29: 113-26.

Kellam, S. G., Ensminger, M. E., and Turner, J. T. 1982. "The Long-Term Evolution of Family Structure of Teenage and Older Mothers." *Journal of Marriage and the Family* 44: 549-54.

Kennedy, P. 1992. *A Guide to Econometrics*. Oxford, U.K.: Martin Robertson, 1979. Reprint, Oxford, U.K.: Blackwell (page references are to reprint edition).

Ketterlinus, R. D., Lamb, M. E., and Nitz, K. 1991. "Developmental and Ecological Sources of Stress Among Adolescent Parents." *Family Relations* 40: 435-41.

Kinard, E. M., and Klerman, L. V. 1983. "Effects of Early Parenthood on the Cognitive Development of Children." In *Premature Adolescent Pregnancy and Parenthood*, ed. E. R. McAnarney. New York: Grune & Stratton.

Kisker, E., Hofferth, S. L., Phillips, D., and Farquhar, E. 1991. *A Profile of Child Care Settings: Early Education and Care in 1990*. Vol. 1. Princeton, N.J.: Mathematica Policy Research.

Koenig, M. A., and Zelnik, M. 1982. "Repeat Pregnancies Among Metropolitan-Area Teenagers." *Family Planning Perspectives* 14: 341-44.

Landy, S., Clark, C., Schubert, J., and Jillings, C. 1983. "Mother-Infant Interactions of Teenage Mothers as Measured at Six Months in a Natural Setting." *Journal of Psychology* 115: 245-58.

Levin-Epstein, J. 1993. *Teen Parents and JOBS: 1992 Statistical Snapshot*. Washington, D.C.: Center for Law and Social Policy.

Levine, L., Garcia-Coll, C. T., and Oh, W. 1985. "Determinants of Mother-Infant Interaction in Adolescent Mothers." *Pediatrics* 75: 23-29.

Levy, F., and Michel, R. 1988. "Education and Income: Recent U.S. Trends." Research paper. Washington, D.C.: Urban Institute.

Levy, S. B. 1983. *Choices and Life Circumstances: An Ethnographic Study of Project Redirection Teens*. New York: Manpower Demonstration Research Corporation.

Long, L. 1992. "International Perspectives on the Residential Mobility of America's Children." *Journal of Marriage and the Family* 54: 861-69.

Luster, T. and Rhoades, K. 1989. "The Relation Between Childrearing Beliefs and the Home Environment in a Sample of Adolescent Mothers." *Family Relations* 38: 317-22.

Lyons-Ruth, K., Connell, D. B., and Grunebaum, H. U. 1990. "Infants at Social Risk: Maternal Depression and Family Support Services as Mediators of Infant Development and Security of Attachment." *Child Development* 61: 85-98.

McGowan, R. J., and Johnson, D. L. 1984. "The Mother-Child Relationship and Other Antecedents of Childhood Intelligence." *Child Development* 55: 810-20.

McLoyd, V. C. 1990. "The Impact of Economic Hardship on Black Families and Children: Psychological Distress, Parenting, and Socioemotional Development." *Child Development* 61: 311-46.

Mallar, C. D., Thornton, C., and Long, D. 1982. *A Comparative Evaluation of the Benefits and Costs of Job Corps*. Princeton, N.J.: Mathematica Policy Research.

Maloney, T. 1991. *Estimating the Returns to a Secondary Education for Female Dropouts*. Madison: Institute for Research on Poverty, University of Wisconsin-Madison.

Martin, S. L., Ramey, C. T., and Ramey, S. 1990. "The Prevention of Intellectual Impairment in Children of Impoverished Families: Findings of a Randomized Trial of Educational Day Care." *American Journal of Public Health* 80: 844-48.

Martinson, K., and Friedlander, D. 1994. *GAIN: Basic Education in a Welfare-to-Work Program*. New York: Manpower Demonstration Research Corporation.

Martinson, K., and Riccio, J. A. 1989. *GAIN: Child Care in a Weflare Employment Initiative*. New York: Manpower Demonstration Research Corporation.

Maynard, R. 1993. *Building Self-Sufficiency Among Welfare-Dependent Teenage Parents: Lessons from the Teenage Parent Demonstration*. Princeton, N.J.: Mathematica Policy Research.

Maynard, R., Nicholson, W., and Rangarajan, A. 1993. *Breaking the Cycle of Poverty: The Effectiveness of Mandatory Services for Welfare-Dependent Teenage Parents*. Princeton, N.J.: Mathematica Policy Research.

Meyers, L. D., Habicht, J., Johnson, C., and Brownie, C. 1983. "Prevalence of Anemia and Iron Deficiency in Black and White Women in the U.S." *American Journal of Public Health* 73: 1042-49.

Mincer, J. 1974. *Schooling, Experience, and Earnings*. New York: National Bureau of Economic Research.

Moore, K. A., and Snyder, N. O. 1991. "Cognitive Attainment Among Firstborn Children of Adolescent Mothers." *American Sociological Review* 56: 612-26.

Moore, K. A. 1993. "Facts at a Glance." Washington, D.C.: Child Trends.

Mosena, P. W. 1986. *Adolescent Parent Outreach Follow-Up Survey*. Chicago: University of Chicago.

Mosher, W. D. 1990. "Contraceptive Practice in the United States." *Family Planning Perspectives* 22: 198-205.

Mott, F. L. 1986. "The Pace of Repeated Childbearing Among Young American Mothers." *Family Planning Perspectives* 18: 5-12.

Mott, F. L., and Quinlan, S. V. 1991. *Maternal-Child Health Data from the NLSY*. Columbus, Ohio: Center for Human Resource Research, Ohio State University.

Murnane, R. J., and Willett, J. B. 1993. *Do High School Dropouts Benefit from Obtaining a GED? Using Multi-Level Modeling to Examine Longitudinal Evidence from the NLSY*. Cambridge, Mass.: Harvard University Graduate School of Education.

Murnane, R. J., Willett, J. B., and Levy, F. N.d. *The Growing Importance of Cognitive Skills in Wage Determination*. Working paper, Harvard Graduate School of Education.

Murphy, K., and Welch, F. 1989. "Wage Premiums for College Graduates: Recent Growth and Possible Explanations." *Educational Researcher* 18: 17-26.

Musick, J. 1991. "Fifteen, Female, and Poor: Seeking Identity Through Motherhood." Keynote address, 25th anniversary conference of the Erikson Institute, Chicago.

National Center for Health Statistics. 1993. "Advance Report of Final Natality Statistics, 1991." *Monthly Vital Statistics Report* 42 (3): Supplement.

National Research Council, Panel on Adolescent Pregnancy and Childbearing. 1987. *Risking the Future: Adolescent Sexuality, Pregnancy, and Childbearing*, 2 vols. Ed. S. L. Hofferth and C. D. Hayes. Washington, D.C.: National Academy Press.

Nord, C. W., Moore, K. A., Morrison, D. R., Brown, B. and Meyers, D. E. 1992. "Consequences of Teen-Age Parenting." *Journal of School Health* 62 (7): 310-17.

Ostle, B. 1975. *Statistics in Research*, 3d ed. Ames: Iowa State University Press.

Parish, W. L., Hao, L., and Hogan, D. P. 1991. "Family Support Networks, Welfare, and Work Among Young Mothers." *Journal of Marriage and the Family* 53: 203-15.

Parks, P. L., and Arndt, E. K. 1990. "Differences Between Adolescent and Adult Mothers of Infants." *Journal of Adolescent Health* 11: 248-53.

Patterson, C. J., Kupersmidt, J. B., and Vaden, N. A. 1990. "Income Level, Gender, Ethnicity, and Household Composition as Predictors of Children's School-Based Competence." *Child Development* 61: 485-94.

Pavetti, L. A. 1992. "The Dynamics of Welfare and Work: Exploring the Process by Which Young Women Work Their Way Off Welfare." Prepared for the APPAM Annual Research Conference, 29 October, by the Malcolm-Wiener Center for Social Policy, John F. Kennedy School of Government, Harvard University.

Pearlin, L. I., Monaghan, E. G., Lieberman, M. A., and Mullan, J. T. 1981. "The Stress Process." *Journal of Health and Social Behavior* 22: 337-56.

Phillips, D., McCartney, K., and Scarr, S. 1987. "Child Care Quality and Children's Social Development." *Developmental Psychology* 23: 537-43.

Pianta, R. C., Sroufe, L. A., and Egeland, B. 1989. "Continuity and Discontinuity in Maternal Sensitivity at 6, 24, and 42 Months in a High-Risk Sample." *Child Development* 60: 481-87.

Pleck, J. H., Sonenstein, F. L., and Ku, L. 1993. "Changes in Adolescent Males' Use of and Attitudes Toward Condoms, 1988-1991." *Family Planning Perspectives* 25: 106-9.

Polit, D. F. 1992a. *The Effect of Maternal Age at First Birth on the Home Environments of Children.* Saratoga Springs, N.Y.: Humanalysis.

Polit, D. F. 1992b. *Barriers to Self-Sufficiency and Avenues to Success Among Teenage Mothers.* Princeton, N.J.: Mathematica Policy Research.

Polit, D. F., Kahn, J., and Stevens, D. 1985. *Final Impacts from Project Redirection: A Program for Pregnant and Parenting Teens.* New York: Manpower Demonstration Research Corporation.

Polit, D. F., Quint, J. C., and Riccio, J. A. 1988. *The Challenge of Serving Teenage Mothers: Lessons from Project Redirection.* New York: Manpower Demonstration Research Corporation.

Polit, D., and White, C. M. 1988. *The Lives of Young Disadvantaged Mothers: The Five-Year Follow-up of the Project Redirection Sample.* Saratoga Springs, N.Y.: Humanalysis.

Quint, J. C., Fink, B. L. and Rowser, S. L. 1991. *New Chance: Implementing a Comprehensive Program for Disadvantaged Young Mothers and Their Children.* New York: Manpower Demonstration Research Corporation.

Quint, J. C., and Musick, J. S. 1994. *Lives of Promise, Lives of Pain: Young Mothers After New Chance.* New York: Manpower Demonstration Research Corporation.

Quint, J. C., and Riccio, J. A. 1985. *The Challenge of Serving Pregnant and Parenting Teens: Lessons from Project Redirection.* New York: Manpower Demonstration Research Corporation.

Radloff, L. S. 1977. "The CES-D Scale: A Self-Report Depression Scale for Research in the General Population." *Applied Psychological Measurement* 1: 385-401.

Richters, J., and Pellegrini, D. 1989. "Depressed Mothers' Judgments About Their Children: An Examination of the Depression-Distortion Hypothesis." *Child Development* 60: 1068-75.

Roosa, M. 1983. "A Comparative Study of Pregnant Teenagers' Parenting Attitudes and Knowledge of Sexuality and Child Development." *Journal of Youth and Adolescence* 12: 213-23.

Rotter, J. B. 1966. "Generalized Expectancies for Internal Versus External Control of Reinforcement." *Psychological Monographs* 66 (80).

Scheirer, M. A. 1983. "Household Structure Among Welfare Families: Correlates and Consequences." *Journal of Marriage and the Family* 45: 761-71.

Schoenborn, C. A. 1986. "Health Habits of U.S. Adults, 1985." *Public Health Reports* 101: 571-80.

Simons, R. L., Beaman, J., Conger, R. D., and Chao, W. 1993. "Stress, Support, and Antisocial Behavior Trait as Determinants of Emotional Well-Being and Parenting Practices Among Single Mothers." *Journal of Marriage and the Family* 55: 385-98.

Smith, S. 1991. "Two-Generational Program Models: A New Intervention Strategy." *Social Policy Report* 5 (1). Ann Arbor: Society for Research in Child Development, University of Michigan.

Strobino, D. M., Alexander, C. S., and Kim, Y. J. 1988. *Factors Associated with Accidental Injuries in Urban Children Aged 1-9 Years*. Final Report to DMCH. Baltimore: Johns Hopkins University.

Takeuchi, D. T., Williams, D. R., and Adair, R. K. 1991. "Economic Stress in the Family and Children's Emotional and Behavioral Problems." *Journal of Marriage and the Family* 53:1031-41.

Testa, M. F. 1992. "Racial and Ethnic Variation in the Early Life Course of Adolescent Welfare Mothers." In *Early Parenthood and Coming of Age in the 1990s*, ed. M. K. Rosenheim and M. F. Testa (89-112). New Brunswick, N.J.: Rutgers University Press.

Thornton, A., Young-DeMarco, L., and Goldsheider, F. 1993. "Leaving the Parental Nest: The Experience of a Young White Cohort in the 1980s." *Journal of Marriage and the Family* 55: 216-29.

Upchurch, D. M. 1988. "The Effects of Early Childbearing on High School Completion Among Recent Cohorts of American Women." Ph.D. diss., Johns Hopkins University, Baltimore, Maryland.

Upchurch, D., and McCarthy, J. 1990. "The Timing of a First Birth and High School Completion." *American Sociological Review* 55: 224-34.

U.S. Bureau of the Census. 1991. *State and Metropolitan Area Data Book, 1991.* Washington, D.C.: U.S. Bureau of the Census.

U.S. Bureau of the Census. 1993. *Statistical Abstract of the United States, 1993* (113th edition). Washington, D.C.: U.S. Bureau of the Census.

U.S. Congress, House Committee on Ways and Means. 1990. *Overview of Entitlement Programs*. Washington, D.C.: U.S. Government Printing Office.

U.S. Congress, House Committee on Ways and Means. 1993. *Overview of Entitlement Programs*. Washington, D.C.: U.S. Government Printing Office.

U.S. Department of Education, National Center for Education Statistics. 1988. *Education Indicators*. Washington, D.C.: U.S. Department of Education.

U.S. General Accounting Office. 1986. *Teenage Pregnancy: 500,000 Births a Year but Few Tested Programs*. Washington, D.C.: U.S. General Accounting Office.

Vega, W. A., Kolody, B., Valle, R., and Hough, R. 1986. "Depressive Symptoms and Their Correlates Among Immigrant Mexican Women in the United States." *Social Science and Medicine* 22: 645-52.

Whitebook, M., Howes, C. and Phillips, D. 1990. *Who Cares? Child Care Teachers and the Quality of Care in America*. Final report of the National Child Care Staffing Study. Oakland, Calif.: Child Care Employee Project.

Winpisinger, K. A., Hopkins, R. S., Indian, R. W., and Hostetler, J. R. 1991. "Risk Factors for Childhood Homicides in Ohio." *American Journal of Public Health* 81: 1052-54.

Yamaguchi, K., and Kandel, D. 1984. "Patterns of Drug Use from Adolescence to Young Adulthood." *American Journal of Public Health* 74: 673-81.

SELECTED MDRC PUBLICATIONS

PROGRAMS FOR TEENAGE PARENTS ON WELFARE

The New Chance Demonstration

A test of a comprehensive program of services that seeks to improve the economic status and general well-being of a group of highly disadvantaged young women and their children.

New Chance: Implementing a Comprehensive Program for Disadvantaged Young Mothers and Their Children. 1991. Janet Quint, Barbara Fink, Sharon Rowser.

Lives of Promise, Lives of Pain: Young Mothers After New Chance. 1994. Janet Quint, Judith Musick, with Joyce Ladner.

New Chance: An Innovative Program for Young Mothers and Their Children. Brochure. 1993.

The LEAP Evaluation

An evaluation of Ohio's Learning, Earning, and Parenting (LEAP) Program, which uses financial incentives to encourage teenage parents on welfare to stay in or return to school.

LEAP: Implementing a Welfare Initiative to Improve School Attendance Among Teenage Parents. 1991. Dan Bloom, Hilary Kopp, David Long, Denise Polit.

LEAP: Interim Findings on a Welfare Initiative to Improve School Attendance Among Teenage Parents. 1993. Dan Bloom, Veronica Fellerath, David Long, Robert Wood.

Project Redirection

A test of a comprehensive program of services for pregnant and parenting teenagers.

Project Redirection: Interim Report on Program Implementation. 1981. Alvia Branch, Janet Quint.

Needs and Characteristics of Pregnant and Parenting Teens: The Baseline Report for Project Redirection. 1982. Denise Polit.

Choices and Life Circumstances: An Ethnographic Study of Project Redirection Teens. 1983. Sydelle Levy.

School, Work and Family Planning: Interim Impacts in Project Redirection. 1983. Denise Polit, Michael Tannen, Janet Kahn.

Building Self-Sufficiency in Pregnant and Parenting Teens: Final Implementation Report of Project Redirection. 1984. Alvia Branch, James Riccio, Janet Quint.

Final Impacts from Project Redirection: A Program for Pregnant and Parenting Teens. 1985. Denise Polit, Janet Kahn, David Stevens.

Strengthening Services for Teen Mothers. 1985. James Riccio.

Training for Transition: A Guide for Training Young Mothers in Employability Skills. 1985. Elizabeth McGee.

The Challenge of Serving Teenage Mothers: Lessons from Project Redirection. Monograph. 1988. Denise Polit, Janet Quint, James Riccio.

The Community Service Projects

A test of a New York State teenage pregnancy prevention and services initiative.

The Community Service Projects: A New York State Adolescent Pregnancy Initiative. 1986. Cynthia Guy.

The Community Service Projects: Final Report on a New York State Adolescent Pregnancy Prevention and Services Program. 1988. Cynthia Guy, Lawrence Bailis, David Palasits, Kay Sherwood.

PROGRAMS FOR YOUTH

The School-to-Work Transition Project

A study of innovative programs that help students make the transition from school to work.

The School-to-Work Transition and Youth Apprenticeship: Lessons from the U.S. Experience. 1993. Thomas Bailey, Donna Merritt.
Home-Grown Lessons: Innovative Programs Linking Work and High School. 1994. Edward Pauly, Hilary Kopp, Joshua Haimson.
Learning Through Work: Designing and Implementing Quality Worksite Learning for High School Students. 1994. Susan Goldberger, Richard Kazis, Mary Kathleen O'Flanagan (all of Jobs for the Future).

The JOBSTART Demonstration
A test of a program combining education, training, support services, and job placement for very disadvantaged young high school dropouts.

The Pilot Phase: A Case Study of Five Youth Training Programs. 1985. Michael Redmond.
Launching JOBSTART: A Demonstration for Dropouts in the JTPA System. 1987. Patricia Auspos.
Implementing JOBSTART: A Demonstration for School Dropouts in the JTPA System. 1989. Patricia Auspos, George Cave, Fred Doolittle, Gregory Hoerz.
Assessing JOBSTART: Interim Impacts of a Program for School Dropouts. 1991. George Cave, Fred Doolittle.
JOBSTART: Final Report on a Program for School Dropouts. 1993. George Cave, Hans Bos, Fred Doolittle, Cyril Toussaint.

The Career Beginnings Evaluation
An evaluation of a program that seeks to increase college attendance and improve job quality among disadvantaged high school students.

Career Beginnings Impact Evaluation: Findings from a Program for Disadvantaged High School Students. 1990. George Cave, Janet Quint.

The Youth Incentive Entitlement Pilot Projects (YIEPP) Demonstration
A test of a school-conditioned job guarantee for low-income youth.

Lessons from a Job Guarantee: The Youth Incentive Entitlement Pilot Projects. Monograph. 1984. Judith Gueron.

WELFARE-TO-WORK PROGRAMS

From Welfare to Work (Russell Sage Foundation). Book. 1991. Judith M. Gueron, Edward Pauly. A synthesis of research findings on the effectiveness of welfare-to-work programs. Chapter 1, which is the summary of the book, is also published separately by MDRC.
Reforming Welfare with Work (Ford Foundation). Monograph. 1987. Judith M. Gueron. A review of welfare-to-work initiatives in five states.

Papers for Practitioners

Assessing JOBS Participants: Issues and Trade-offs. 1992. Patricia Auspos, Kay Sherwood.
Linking Welfare and Education: A Study of New Programs in Five States. 1992. Edward Pauly, David Long, Karin Martinson.
Improving the Productivity of JOBS Programs. 1993. Eugene Bardach.

Working Papers

Child Support Enforcement: A Case Study. 1993. Dan Bloom.
Learning from the Voices of Mothers: Single Mothers' Perceptions of the Trade-offs Between Welfare and Work. 1993. LaDonna Pavetti.

Unpaid Work Experience for Welfare Recipients: Findings and Lessons from MDRC Research. 1993. Thomas Brock, David Butler, David Long.

The Impacts of California's GAIN Program on Different Ethnic Groups: Two-Year Findings on Earnings and AFDC Payments. 1994. Daniel Friedlander.

The GAIN Evaluation

An evaluation of California's Greater Avenues for Independence (GAIN) Program, which is currently operating as the state's JOBS program and features upfront basic education as well as job search and other activities.

GAIN: Planning and Early Implementation. 1987. John Wallace, David Long.

GAIN: Child Care in a Welfare Employment Initiative. 1989. Karin Martinson, James Riccio.

GAIN: Early Implementation Experiences and Lessons. 1989. James Riccio, Barbara Goldman, Gayle Hamilton, Karin Martinson, Alan Orenstein.

GAIN: Participation Patterns in Four Counties. 1991. Stephen Freedman, James Riccio.

GAIN: Program Strategies, Participation Patterns, and First-Year Impacts in Six Counties. 1992. James Riccio, Daniel Friedlander.

GAIN: Two-Year Impacts in Six Counties. 1993. Daniel Friedlander, James Riccio, Stephen Freedman.

GAIN: Basic Education in a Welfare-to-Work Program. 1994. Karin Martinson, Daniel Friedlander.

GAIN: Benefits, Costs, and Three-Year Impacts of a Welfare-to-Work Program. 1994. James Riccio, Daniel Friedlander, Stephen Freedman.

Related Study:

The Impacts of California's GAIN Program on Different Ethnic Groups: Two-Year Findings on Earnings and AFDC Payments. Working Paper. 1994. Daniel Friedlander.

The JOBS Evaluation

An evaluation of welfare-to-work programs operating under the Job Opportunities and Basic Skills Training (JOBS) provisions of the Family Support Act of 1988.

From Welfare to Work (Russell Sage Foundation). Book. 1991. Judith M. Gueron, Edward Pauly. See description above.

The Evaluation of Florida's Project Independence

An evaluation of Florida's JOBS program.

Florida's Project Independence: Program Implementation, Participation Patterns, and First-Year Impacts. 1994. James Kemple, Joshua Haimson.

The Saturation Work Initiative Model (SWIM)

A test of the feasibility and effectiveness of an ongoing participation requirement in a welfare-to-work program.

Interim Report on the Saturation Work Initiative Model in San Diego. 1988. Gayle Hamilton.

Final Report on the Saturation Work Initiative Model in San Diego. 1989. Gayle Hamilton, Daniel Friedlander.

The Saturation Work Initiative Model in San Diego: A Five-Year Follow-up Study. 1993. Daniel Friedlander, Gayle Hamilton.

The Demonstration of State Work/Welfare Initiatives

A test of the feasibility and effectiveness of various state employment initiatives for welfare recipients.

Arizona: *Preliminary Management Lessons from the WIN Demonstration Program.* 1984. Kay Sherwood.

Arkansas: *Interim Findings from the Arkansas WIN Demonstration Program.* 1984. Janet Quint.

Final Report on the WORK Program in Two Counties. 1985. Daniel Friedlander, Gregory Hoerz, Janet Quint, James Riccio.

Employment and Welfare Impacts of the Arkansas WORK Program: A Three-Year Follow-up Study in Two Counties. 1988. Daniel Friedlander, Barbara Goldman.

California: *Preliminary Findings from the San Diego Job Search and Work Experience Demonstration.* 1984. Barbara Goldman, Judith Gueron, Joseph Ball, Marilyn Price.

Findings from the San Diego Job Search and Work Experience Demonstration. 1985. Barbara Goldman, Daniel Friedlander, Judith Gueron, David Long.

Final Report on the San Diego Job Search and Work Experience Demonstration. 1986. Barbara Goldman, Daniel Friedlander, David Long.

Illinois: *Interim Findings from the WIN Demonstration Program in Cook County.* 1986. Janet Quint, Cynthia Guy.

Final Report on Job Search and Work Experience in Cook County. 1987. Daniel Friedlander, Stephen Freedman, Gayle Hamilton, Janet Quint.

Maine: *Interim Findings from a Grant Diversion Program.* 1985. Patricia Auspos.

Final Report on the Training Opportunities in the Private Sector Program. 1988. Patricia Auspos, George Cave, David Long.

Maryland: *Interim Findings from the Maryland Employment Initiatives Programs.* 1984. Janet Quint.

Final Report on the Employment Initiatives Evaluation. 1985. Daniel Friedlander, Gregory Hoerz, David Long, Janet Quint.

Supplemental Report on the Baltimore Options Program. 1987. Daniel Friedlander.

New Jersey: *Final Report on the Grant Diversion Project.* 1988. Stephen Freedman, Jan Bryant, George Cave.

Virginia: *Interim Findings from the Virginia Employment Services Program.* 1985. Marilyn Price.

Final Report on the Virginia Employment Services Program. 1986. James Riccio, George Cave, Stephen Freedman, Marilyn Price.

West Virginia: *Interim Findings on the Community Work Experience Demonstrations.* 1984. Joseph Ball.

Final Report on the Community Work Experience Demonstrations. 1986. Daniel Friedlander, Marjorie Erickson, Gayle Hamilton, Virginia Knox.

Other Reports on the Demonstration of State Work/Welfare Initiatives

Documentation of the Data Sources and Analytical Methods Used in the Benefit-Cost Analysis of the EPP/EWEP Program in San Diego. 1985. David Long, Virginia Knox.

Relationship Between Earnings and Welfare Benefits for Working Recipients: Four Area Case Studies. 1985. Barbara Goldman, Edward Cavin, Marjorie Erickson, Gayle Hamilton, Darlene Hasselbring, Sandra Reynolds.

Welfare Grant Diversion: Early Observations from Programs in Six States. 1985. Michael Bangser, James Healy, Robert Ivry.

A Survey of Participants and Worksite Supervisors in the New York City Work Experience Program. 1986. Gregory Hoerz, Karla Hanson.

Welfare Grant Diversion: Lessons and Prospects. 1986. Michael Bangser, James Healy, Robert Ivry.

Work Initiatives for Welfare Recipients: Lessons from a Multi-State Experiment. 1986. Judith Gueron.

The Subgroup/Performance Indicator Study

A study of the impacts of selected welfare-to-work programs on subgroups of the AFDC caseload.

A Study of Performance Measures and Subgroup Impacts in Three Welfare Employment Programs. 1987. Daniel Friedlander, David Long.

Subgroup Impacts and Performance Indicators for Selected Welfare Employment Programs. 1988. Daniel Friedlander.

The Self-Employment Investment Demonstration (SEID)

A test of the feasibility of operating a program to encourage self-employment among recipients of AFDC.

Self-Employment for Welfare Recipients: Implementation of the SEID Program. 1991. Cynthia Guy, Fred Doolittle, Barbara Fink.

The WIN Research Laboratory Project

A test of innovative service delivery approaches in four Work Incentive Program (WIN) offices.

Immediate Job Search Assistance: Preliminary Results from the Louisville WIN Research Laboratory Project. 1980. Barbara Goldman.

Preliminary Research Findings: WIN Research Laboratory Project. 1980. MDRC.

Final Report on WIN Services to Volunteers: Denver WIN Research Laboratory Project. 1981. Ellen Slaughter, Paulette Turshak, Gale Whiteneck, Edward Baumheier.

Impacts of the Immediate Job Search Assistance Experiment: Louisville WIN Research Laboratory Project. 1981. Barbara Goldman.

The Workings of WIN: A Field Observation Study of Three Local Offices. 1981. Sydelle Levy.

Welfare Women in a Group Job Search Program: Their Experiences in the Louisville WIN Research Laboratory Project. 1982. Joanna Gould-Stuart.

The WIN Labs: A Federal/Local Partnership in Social Research. 1982. Joan Leiman.

Job Search Strategies: Lessons from the Louisville WIN Laboratory. 1983. Carl Wolfhagen, Barbara Goldman.

THE PARENTS' FAIR SHARE DEMONSTRATION

A demonstration aimed at reducing child poverty by increasing the job-holding, earnings, and child support payments of unemployed, noncustodial parents (usually fathers) of children receiving public assistance.

Caring and Paying: What Fathers and Mothers Say About Child Support. 1992. Frank Furstenberg, Jr., Kay Sherwood, Mercer Sullivan.

Child Support Enforcement: A Case Study. Working Paper. 1993. Dan Bloom.

Matching Opportunities to Obligations: Lessons for Child Support Reform from the Parents' Fair Share Pilot Phase. 1994. Dan Bloom, Kay Sherwood.

THE NATIONAL JTPA STUDY

A study of 16 local programs under the Job Training Partnership Act (JTPA), the nation's job training system for low-income individuals.

Implementing the National JTPA Study. 1990. Fred Doolittle, Linda Traeger.

The National JTPA Study: Site Characteristics and Participation Patterns. 1993. James Kemple, Fred Doolittle, John Wallace.

A Summary of the Design and Implementation of the National JTPA Study. 1993. Fred Doolittle.

About MDRC

The Manpower Demonstration Research Corporation (MDRC) is a nonprofit social policy research organization founded in 1974 and located in New York City and San Francisco. Its mission is to design and rigorously field-test promising education and employment-related programs aimed at improving the well-being of disadvantaged adults and youth, and to provide policymakers and practitioners with reliable evidence on the effectiveness of social programs. Through this work, and its technical assistance to program administrators, MDRC seeks to enhance the quality of public policies and programs. MDRC actively disseminates the results of its research through its publications and through interchange with policymakers, administrators, practitioners, and the public.

Over the past two decades – working in partnership with more than forty states, the federal government, scores of communities, and numerous private philanthropies – MDRC has developed and studied more than three dozen promising social policy initiatives.

Manpower Demonstration Research Corporation

Three Park Avenue
New York, New York 10016
(212) 532-3200

88 Kearny Street, Suite 1650
San Francisco, California 94108
(415) 781-3800

MDRC

BEST COPY AVAILABLE